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THE PHILOSOPHICAL
HISTORY
AND
MEMOIRS
OF THE

Royal Academy of Sciences at *Paris* :

OR,

AN ABRIDGMENT of all the PAPERS relating
to *Natural Philosophy*, which have been
publish'd by the *Members* of that *Illustrious*
Society.

With many Curious OBSERVATIONS relating to the
Natural History and Anatomy of Animals, &c.

Illustrated with COPPER-PLATES.

The Whole Translated and Abridged,
By JOHN MARTYN, F.R.S.

Professor of Botany in the University of *Cambridge* ;

AND

EPHRAIM CHAMBERS, F.R.S.

Author of the Universal Dictionary of Arts and Sciences.

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Printed for JOHN and PAUL KNAPTON, in *Ludgate-street* ;
and FRANCIS COGAN, and JOHN NOURSE, near *Temple-*
Bar. M.DCC.XLII.

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A T A B L E

OF THE
PAPERS contained in the ABRIDGMENT
of the HISTORY and MEMOIRS of the
ROYAL ACADEMY of SCIENCES at
PARIS, for the Year MDCCLIII.

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OF
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FROM THE YEAR 1666 TO 1793.

WITH
MANY
ORIGINAL
DOCUMENTS
AND
ILLUSTRATIONS
BY
M. DE LA HARPE

THE
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Philosophical Discoveries

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IN THE
HISTORY of the ROYAL ACADEMY of
SCIENCES at *Paris*, for the Year 1703.

I. *On the ways which several bodies turn ;
translated by Mr. Chambers.*

TIS not only understanding that we want
for the discovery of natural causes, but
we seem also to want eyes for the per-
ceiving of effects, and a thousand things are con-
tinually presenting themselves, which we never
see.

How few people have observed, that the shells
of snails, which are spirals wound about a kind
of cone, are all wound the same way in the same
species ; and what is more, the same way in al-
most all species is from left to right, considering
them with the point upwards. *M. Parent*, after
several enquiries among all the land, river, sea,
and even stone snails, has only found three kinds,
whose shells are turned from right to left.

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The same observation he has extended to plants. There are a multitude of plants, which have either their stems, flowers, or pods twisted a certain way: others, whose flowers, or seeds, are fastened spirally around the stem; and consequently are fastened thereto in a determinate way. Now this way is always the same in the same species; and what is more, *M. Parent*, having made the most complete enumeration of the several kinds of plants, with respect to such properties, has always found, that one certain way prevailed; that is, for instance, that for 33 species of plants, whose stems were twisted to the right, there were only 4 which had untwisted the other ways; and that for 15 species of pods twisted to the left, there were only 2 to the right.

M. Parent has likewise observed, that the fibres of the human heart are always twisted the same way; the external ones from right to left descending, and the internal ones the same way ascending; and that on the contrary, the little tuft, formed by the young hairs on the top of the head, is almost always twisted from left to right, with regard to the person it is on.

It may suffice to have hinted, that there are observations to be made upon such kinds of subjects, commonly so little observed; and that a man may turn his eyes and attention that way, every body hereafter will find the like occasions enough in nature.

The constant and invariable determination in some parts, whether of a plant, or an animal, to be twisted the same way, does doubtless arise from the seed or the egg; and this is a new proof, that generation is only unfolding; but who gave the egg or the seed this determination?

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The same question may be made, as to the way wherein the planets turn ; and the whole may be referred to the arbitrary will of him who made the universe. Another question, *viz.* why a greater number of plants, or shells, are twisted a certain way must be solved from the concatenation of second causes ?

M. *Parent* conjectures, that this may have some dependance on the system of the loadstone. He is convinced, that we may admit two vortices of magnetical matter, which winds spirally around the earth, and along its axis, in two ways opposite to each other ; one whereof issues at one hemisphere, and the other at the opposite one. This magnetical matter is subtile enough to penetrate bodies, and consequently the eggs or seeds of bodies ; and if by any cause whatever, one of the vortices be more disposed to penetrate certain seeds or eggs, than the other, it will twist them in its own way. Each vortex perhaps may commonly have most power in the hemisphere it issues at, on which footing the bodies capable of being thus twisted, should effect a contrary way in the southern hemisphere from what they effect in ours ; but this notion wants to be confirmed by observations.

II. *Of a little insect, called puceron.*

M. *de la Hire* has observed the little insects, called *pucerons*, because they are like little green [*puces*] fleas. They fasten themselves to the young shoots of trees and plants, and destroy a part of them. The leaves under which they place themselves, curl quite up, and serve them at the same time for a covering against the injuries of the air, and for nourishment. They hatch about the middle

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dle of spring, and grow very considerably in about a month. They have 3 feet on each side, and two pretty long horns on their heads. They are of a long rounded shape, the head small, with 2 eyes of a reddish brown. Some time afterwards, they are seen with wings. *M. de la Hire* has found on the plants, which they eat so great a quantity of white spoils, containing the feet and horns of this animal, that he does not question their being coverings, which the *pucerons* quit, which have their horns and legs fastened to them, and hide the little wings, which they must use, after the legs are fallen off. These wings entirely disengage themselves in half an hour; they are then very white, but after they are disengaged, they grow black by degrees, and perfectly like those of the common flies. The head of the insect becomes black also, the body brown, and it diminishes a little in bigness.

Here then is a species of insects, which, from walking, come to fly, without passing as most other flying insects do, thro' the state of an *aurelia*, or *chrysalis*. Instead of passing thro' this metamorphosis, these *pucerons* quit their covering; it perfectly resembles that with which the frogs are cloathed, when they are in the state of tadpoles, incapable of walking on the ground, and fit only to swim.

M. de la Hire thinks the *pucerons* live a whole year, and that, during the winter, they retire into holes, whence they come out in the spring to lay their eggs, as the common flies do.

The ants are very fond of the *pucerons*. They gather in great quantities on the plants where they are, and the diseases of these plants are imputed to them; but they are *pucerons* that have done
all

all the harm; and the ants have only drawn them thither.

III. *Of the yellow amber.*

M. Galland, of the academy of inscriptions, has confirmed the academy of sciences in what was said of the yellow amber, in the history of 1700 *. He found some of it at *Marseilles*, on the sea shore, in a place where there were no trees, and where the sea was bounded only by very steep rocks, which were beaten by the waves in rough weather. The yellow amber must have been detached from the clefts of these rocks, whence it fell into the sea.

IV. *Of a valuable sort of alabaster.*

At the same time M. Galland informed the academy, that he had seen at the seat of the illustrious M. Puget, of *Marseilles*, some columns of alabaster of different colours, and very precious. It is so transparent, that thro' the very perfect polish of which it is capable, one may see above two fingers into its thickness the agreeable variety of colours with which it is embellished. M. Puget told M. Galland, that he was the only person who knew the quarry, tho' it was not far from *Marseilles*.

V. *Of a bottle of water kept for a great many years.*

M. Dodart shewed a bottle of water of *Sainte Reine*, kept ever since the year 1678, without any corruption, or any sediment that appeared at

* See Vol. I. pag. 186 of this abridgment.

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the bottom. There was however a little air in the bottle. The wicker of it was rotten. Upon breaking it, there was found at the bottom a light earthy sediment, which had nothing saline in it. Some only were of opinion, that this earth might be a little tartarous.

VI. *Of a surprising recovery from deafness and dumbness.*

M. Felibien, of the academy of inscriptions, informed the academy of sciences of a singular and perhaps unheard of accident, which happened at Chartres. A young man of 23 or 24 years of age, son of an artisan, who had been deaf and dumb from his birth, began all at once to speak, to the great astonishment of the neighbourhood. They found that 3 or 4 months before, he had heard the sound of bells, and been extremely surprised with this new and unknown sensation. Afterwards there came a sort of water out of his left ear, and he heard perfectly with both ears. He spent 3 or 4 months in hearing, without speaking at all, accustoming himself to repeat low the words which he heard, and confirming himself in the pronounciation, and in the ideas annexed to words. At last, he thought himself fit to break silence, and declared that he spoke, tho' it was yet but imperfectly. Some able divines immediately asked him about his past state, and their principal questions turned upon God, the soul, and the moral goodness or evil of actions. He did not seem to have carried his thoughts so far. Tho' he was born of catholic parents, went to mass, was instructed to make the sign of the cross, and to kneel in a posture of praying; yet he never added any intention to all this,

this, nor comprehended what others intended thereby. He did not know distinctly what death was, and had never thought about it. He led a mere animal life, entirely engaged upon sensible and present objects, and on the few ideas which he received by his eyes. He did not even draw from the comparison of these ideas all, that one would have thought, he might draw from it. Not that he had naturally any want of understanding, but the understanding of a man deprived of the conversation of others, is so little exercised, and so little cultivated, that he does not think so much thereon, as he is indispensably obliged by external objects. The greatest fund of the ideas of men is in their reciprocal conversation.

VII. *Of a storm of hail of an extraordinary bigness.*

M. Parent related, that May 15, there fell about *Iliers*, in *le Perche*, a prodigious quantity of hail, which was also of a prodigious bigness. The least was as big as one's two thumbs, and the biggest were of the size of one's fist, and weighed five quarters of a pound, and the middle size was like pullets' eggs, and in much greater quantity. It fell in several places a foot deep. There were 30 parishes, in which the corn was cut as it were with a scythe. The inhabitants of *Iliers*, seeing this ravage, had recourse to their bells, which they rung so briskly, that the cloud was parted over their parish into two parts, and separated in such a manner, that their parish alone, in the midst of 30 others, which had not such good bells, was very little injured. M. Parent related farther, that as the corn was then not very forward, tho' for the most part

in the ear, it made new shoots from the bottom, and that these stalks began to form little spikes, which they hoped would come to maturity; we have since heard that they had a good harvest.

VIII. *A magnetical experiment.*

A plate of steel touched with a loadstone, sustains a greater weight when it is longer. M. *Descartes*, and after him M. *Robaut*, have thought, that this increase of force came from the magnetical matter acquiring more quickness in passing thro' a longer plate, because it finds the ways more easy than otherwise. M. *Joblot*, who has studied the loadstone particularly, and made a great many discoveries on this subject, in a discourse one day with M. *Carré*, supposing this augmentation of the quickness of the magnetical matter, found him not much disposed to embrace this opinion. As M. *Carré* was disputing against it, there came an experiment into his mind, which would clear up the truth. It was to make three plates of good steel, well polished, an inch broad or thereabouts, one of which should be twice the length of the other two, and of the same weight with both of them together, to draw them an equal number of times on the loadstone, that they may be touched as equally as possible, and to see afterwards what weight the longest would sustain alone, and the two small ones put one upon another, so that the poles of the same name might be together, for otherwise they would have no effect. If the increase of force of one longer plate came from the magnetic matter, having increased its quickness in passing thro' it, then the long plate must sustain a
I greater

greater weight than the two small ones ; but if this augmentation of force came, as M. Carré thought, only from a greater quantity of magnetic matter passing thro' a longer plate, then the two small ones must sustain as much as the great one. The experiment was made by M. Joblot, and the longest plate sustained eight ounces two drams ; and the two smallest, a little more than seven ounces ; their not going beyond eight ounces two drams was occasioned by their not being wrought enough to join exactly ; for we know that the more two plates are united, the more force they have.

To this experiment was added a fourth plate, equal in length to the two small ones, but a little heavier than the great one. It had been touched like the other three, and it sustained only a dram more than each of the small ones, which came probably from its thickness, which had rendered it more hard to be penetrated by the magnetic matter. Hence it follows, that among plates of equal thickness, and consequently equally penetrable by the magnetic matter, the longest have the greatest force, only because a greater quantity of this matter has entered into them.

Other experiments, that were made with the same design on the same plates, terminated in the same conclusion.

IX. *Of the effect of vinegar on some stones.*

M. Carré has related, that having gathered some little flat smooth stones among the sand in the river ; he put them into a bason, the bottom of which was a little inclining from the sides to the centre, and placed them at the circumference ; that upon pouring in some vinegar,

all the little stones got presently to the centre. He thinks the reason of this effect to be, that the vinegar causes a dissolution in the stones, and consequently drives out the air which is then under them, raises them up, and makes them roll on an inclined plane. For the same reason the star-stone turns in distilled vinegar, and on a horizontal plane.

X. *The former account of the inaccessible mountain found to be false.*

It was said in the hist. of 1700 *, that if the inaccessible mountain of *Dauphiny*, which has its point at bottom and base at top, was well observed, it might be found to be a mistake. This has really been the case, the academy has been informed both by M. *de Vaubonnays*, first president of the chamber of accounts of *Grenoble*, who is pleased to be their correspondent, and by a letter from M. *Cassat*, secretary to M. *Bouchu*, intendant of *Dauphiny*, to M. *de la Hire*, that this pretended inaccessible mountain, which is eight or nine leagues from *Grenoble* to the S. is nothing but a steep rock planted on the top of an ordinary mountain, and that even this rock has not the figure of an inverted pyramid. Besides, there is no appearance of any rock or part of a rock having changed the figure, which this marvellous mountain might have in the time of *Charles VIII.* for it is between some mountains of a living rock, and we do not find at the bottom any ruins of rocks, as we do in many other places.

* See Vol. I. pag. 181 of this abridgment.

XI. *Another fiction of an uncommon shining of the sea.*

There came a letter from *Cadiz*, importing, that for 15 nights together they had seen the whole sea shining with a clear light, almost like a liquid *Phosphorus*; and to make this comparison the more perfect, that the sea water being carried away in bottles, gave the same light in the dark; that some drops of it being let fall on the ground shone like sparks of fire, and that linnen dipt in this water became also luminous. The fact having been well examined, is found to be false. At most, this report, which was spread even in *Spain*, might be founded on some particular and lively colour, which the sea might have at sun-setting. The academy think, they do as much service to the publick, in disabusing them with regard to false wonders, as in recounting to them the true.

XII. *Of some petrified bodies found inclosed in stones.*

M. *Maraldi* has brought from *Italy* some hard stones, of a whiteish colour, which split into leaves, and discover dried fishes, chaff and olive leaves. They were found in the *Veronese*, by the chevalier *Bianchi*. It has happened luckily, that in cleaving the greatest part of those which contained a petrified fish, it has been split in the middle of its thickness, in such a manner that the two parts are very easy to be known. They seem as if imprinted in a mould. All the outer part of the body of the animal is very exactly marked, and there is no room to question their being real fishes,

fishes, involved in a sand which afterwards petrified.

M. *Maraldi* saw, in the grand duke's gallery, fishes of the same kind dried in stones, which had been taken in *Phœnicia*, in the territory of *Biblis*, now called *Gibeal*, on some mountains almost inaccessible, and 15 miles distant from the sea.

He saw also at *Rome* several large pieces of marble, mixt with red and white, which come from the mountains of *Sicily*. The white part of them is formed of a great quantity of shells inclosed and incorporated in the marble.

What can have brought these fishes and shells into the earth, and even to the tops of mountains? Probably there are subterraneous fishes, as well as subterraneous waters; and these waters, which, according to M. *de la Hire's* system, rise in vapours, carry with them perhaps some very light eggs and seeds, after which, when they are condensed and replaced in the water, these eggs may hatch, and become fishes or shells. And if these currents of water already raised a good deal above the level of the sea, and perhaps even to the tops of mountains, come by some accident either to dry up, or to take another course among sands, and at last abandon in any manner whatsoever the animals which were nourished in it, they will be left dry, and involved in earth, which petrifying itself, will petrify them also. These waters themselves may be petrified, after having passed thro' certain earths, and be loaded with certain salts. If all stones have been liquid, as some great philosophers think, then this system is the more probable.

XIII. *Why the bottom of a pot is not so hot, when the water contained in it boils, as when it does not boil.*

One day a question was asked, why a vessel full of boiling water is less hot at the bottom, whilst the water being taken off the fire continues to boil, than when it ceases to boil; for whilst the water continues to boil, one may touch the bottom of the vessel with one's hand, without burning it, which one cannot do as soon as it has done boiling. We must add, that to make this experiment succeed, the bottom of the vessel must be thin, and the vessel itself pretty large.

M. *Homberg* said, that he conceived a body was hot, only because it was penetrated every way by the matter of flame or of light, which coming out of all parts with impetuosity, like an infinite number of very sharp little darts, carried the impressions of the heat into all the bodies that it struck; that when a vessel is on the fire, the flame being driven upwards by the weight of the air, endeavoured to make passages for itself into the water of the vessel; according to this direction, that at first it must have found it difficult to penetrate this water, where there were no such passages as its motion required, that it made them at last, and in this state the water boiled, that then all the passages upwards thro' the water were easy, the flame, which incessantly struck the bottom of the vessel, drove them easily thro', that consequently the vessel being taken off the fire, the motion of all the little darts was at that instant upwards, and so they could not hurt the hand which touched the vessel; but that when it had done boiling, all its parts being less agitated, sinking and falling one upon another, closed an infinite

infinite number of passages, that before were open, which obliged the little darts to escape indifferently on all sides, and that consequently the hand being applied to the bottom of the vessel, must receive a great number and be hurt by them.

The bottom of the vessel is regarded here only as a mere surface, which the flame passes thro' only before it arrives at the water, and on which it does not act. But if this bottom has any thing of a considerable thickness, the flame necessarily acts upon it, as it penetrates, not only upwards, according to the direction impressed by the weight of the air, but also downwards, and according to all the determinations, because it is reflected by the solid parts; and thence it comes, that in this case, independantly on the different states of the water, the hand will always perceive heat in touching the bottom. If it is thin, and at the same time the vessel is very small, then the sides of the vessel, which almost encompass the bottom, communicate their heat to it, and consequently the vessel can no more be too big, than the bottom can be too thin.

XIV. *That the heat of water does not increase, after it has boiled.*

This little system of heat has supplied M. *Homberg* with the reason of a surprizing fact, which M. *Amontons* has discover'd in working at his thermometer, and has served him in the construction of it. If water boils as long as it can, its degree of heat does not increase, tho' it is kept longer on the fire, or on a greater fire. Water, according to M. *Homberg*, is hot only because it is penetrated by the matter of the flame, which swells, raises, and roughens its surface; when

when this matter has made free passages on all sides thro' the water, the water boils as much as it can, and the matter of the flame can do nothing farther ; therefore, whether there is a continual succession of new matter, or whether it comes in greater abundance, it neither can open the passages better, nor open a greater quantity of them,

XV. *Of a monstrous lamb.*

M. *Antoine*, surgeon at *Mery* upon the *Seine*, a correspondent in anatomy with M. *Mery*, having met with a very remarkable monster, new consequences were drawn from it, with regard to the circulation between the mother and the *fœtus*, which were communicated to M. *Mery*, and by him to the academy.

A sheep brought forth two lambs, one of which was alive, well formed in all its parts, at its full time, and the other dead and monstrous, without either head, breast, *vertebræ*, or tail, having only a sort of belly, at the end of which were the hinder thighs, legs, and feet. This belly was round and oblong, being seven or eight inches in its greatest length, five or six in its greatest breadth, and above three inches in thickness ; it had no aperture, nor any distinction of sex ; it was very fleshy, as well as the thighs, covered with a skin just like that of young lambs. In the middle of the fore-part was the navel-string, composed of four pretty thick vessels, and very well distinguished.

The inside of this deformed trunk was the most wonderful ; it had neither heart, lungs, liver, spleen, kidneys, bladder, spermatic vessels, nor any parts of generation, but only a mesentery

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and intestines, which were fasten'd to it, and a sort of stomach, which however had no shape. This stomach and the intestines contained a yellow and viscid liquor, like excrements.

The umbilical vessels were the origin and end of all the rest; and all the nerves began from a pyramidal body, of the bigness of a walnut, which consequently supplied the place of the brain. The whole fabrick of the mass, or the assemblage of the bones, consisted of a bone situated in the upper part of the monster, of two *ossa ilia*, or *innominata*, which formed a sort of basin, and of the bones of the lower extremities. This pyramidal body, which represented the brain, was fastened to the bone of the upper part.

Two animals fastened together by some common part, or a single animal with those parts double, which are naturally single, would be much less surprising than this half animal; and therefore are not so rare. Two *ova*, meeting in the *uterus*, may stick together, and yet receive nourishment, and then, either all the parts of both unfold and subsist, and they are two animals fastened together; or else some parts of one of them perish, and then remains an animal and a half animal united, in such a manner, that the half animal finds all that it wants in the whole animal. But as for a half animal, which does not join to another, but is deprived of all the most necessary parts, as the heart, lungs, and liver; how could such a one be formed and nourished? how could nature detach a half from a whole, that is so well connected, and so indivisible as an animal? and how could she detach the most depending half from that which go-

verns it, and contains the principal springs of the machine?

It necessarily follows from the want of the heart in this monster, that the impulse of the circulating blood did not proceed from the heart, and consequently M. *Antoine* has sought for another principle of it.

According to the common system, the arteries of the *uterus* pour the blood into the *placenta*, which is nourished thereby; the overplus of this blood enters into the umbilical veins, which make part of the navel-string; thence it is carried to the liver of the *fetus* in the trunk of the *vena portæ*, whence it passes into the *vena cava*, and into the right ventricle of the heart. The blood of the mother being once arrived at the heart of the *fetus*, is afterwards usually distributed into the parts of the *fetus*, except the alterations made in its course by the *foramen ovale*, and the canal of communication. There is yet another necessary difference, that the navel-string and the *placenta* must be comprehended in the circulation; and for this reason, the blood, which comes out of the iliac arteries of the *fetus*, enters the navel-string by the umbilical arteries in the *placenta*, where it is taken up again by the veins of this part, and afterwards by the umbilical veins, which carry it back with new blood from the mother, to the liver and heart of the *fetus*.

As the heart was wanting in M. *Antoine's* monstrous lamb, either the blood of the mother, carried into the *fetus* by the umbilical veins, according to the common system, must have returned to the *placenta* by the umbilical arteries, contrary to the ordinary rules of the circulation, which do not seem to permit this return of the blood by the

arteries, or else this very blood being driven by the mother into the arteries of the *placenta*, must have passed thence into the umbilical arteries, dispersed itself into all the parts of the *fætus*, and returned to the *placenta* by the umbilical veins; which seems more conformable to the laws of circulation, but entirely reverses the common system.

M. *Antoine* has chosen rather to give up this system, than to admit an exception to the ordinary laws of circulation. He has even thought, that if in this monster the blood of the mother entered by the umbilical arteries, it followed the same course in all the *fætus's*, and he sent a paper to the academy, wherein this new opinion was ingeniously explained and supported.

We must not forget to observe in the monstrous lamb, the extraordinary brain, placed in a still more extraordinary manner in the belly, which was the whole animal. We may thence conjecture, how much a common principle of the nerves is necessary and indispensable in the whole animated machine.

XVI. *Of the oriental and occidental bezoar.*

The bezoar is nothing but the gall-stone of several sorts of animals, both of the *East* and *West-Indies*, as goa's, hogs, monkeys, &c. The virtue of the bezoar consists in the volatile alkaline salts of which it is formed, since it is in effect nothing but the bile of these animals. It is thereby that it destroys acids, and promotes perspiration; and without going so far to seek for bezoar, every gall-stone of an animal is such, tho' perhaps more or less active, according to the different animals, and the different climates where they

they live. The bezoar sometimes also gets a little from other parts besides the gall.

XVII. *Of a lizard from the East-Indies.*

F. Gouye shewed a lizard from the *East-Indies*; called by the natives *Phatagen*, and by *Aldrovandus*, *Lacerta Indica Squamosa*. It was two feet $\frac{1}{2}$ long, shaped almost like a crocodile, covered with scales of the size of a crown-piece. His stomach was found full of ants, which are the common food of this animal; it has also a tongue near a foot long, to catch them the more easily. It had in the upper part of the stomach a bag full of living worms, about the thickness and length of pins, in number about 1000, and, which is very astonishing, it was no disease; there were as many found in another animal of the same species. We have already observed the same thing in a tiger from *China*, in the history of 1699*.

XVIII. *Of a puppy, which had only one eye in the middle of its face.*

M. Littre shewed a dead puppy †, which had but one eye, without an eye-lid, situated in the middle of the lower part of the face, just as the *Cyclops* are painted. It had neither nose, nor throat, nor any aperture to supply them. It was fat and well nourished, and died soon after it was born; no doubt because it could not take in any air. This *fetus* could not have been nourished any other way than by the navel.

* Vol. I. Pag. 21, of this Abridgment.

† Plate I. Fig. 1,

string.

string. It had above its eye, and towards the middle of its face, a sort of cartilaginous body, $\frac{1}{2}$ an inch long, and a line thick, solid, of a conical figure, and without any aperture.

XIX. *On the valves of pumps.*

M. *Amontons* having constructed a forcing pump, was surprized to see that the valves, which were of metal, well made, and well ordered, sometimes ceased to play, and stopped on a sudden. He thought some viscid sediment had stuck these valves together, but he found nothing like it, upon examining the pumps several times. The wonder of the effect consisted in this, that, when the piston was raised, there was a vacuum formed between the piston and the valve; that consequently this valve was driven upwards, by the water, which was driven itself by the weight of the whole atmosphere; that the body of the pump being immersed above six feet in water, the weight also of six feet of water pressed the valve, being equal to about $\frac{2}{3}$ of the weight of the atmosphere; and yet that this valve, which was intended to open by this impulse, did not open at all.

This effect may be compared to the known experiment of two surfaces well polished and wetted, applied against each other, which cannot be separated but by the action of a great weight; for the surfaces of the valves are in like manner applied to their shells as exactly as possible, and both of them are necessarily moistened. But there appears at first sight an essential difference, that in the common experiment we may ascribe the union of the two surfaces, or the difficulty of separating them, to the pressure of the air, because,

cause, as there is no air between them, the whole atmosphere resists their separation; on the contrary, with regard to the valves, it is the whole atmosphere; and also a force still greater, that tends to separate them from their shells.

It remains therefore, that the whole force of the union of the valves and shells consists in the water that moistens them; the parts of water, which have entered the pores of one of the bodies, must powerfully fasten upon the other, and every one of them must hold to the two bodies by its two extremities; for free and flowing parts of water would hinder the union, they must fasten so much the more powerfully, as the two surfaces are more polished, better wrought, and exclude the air more perfectly; the multitude of the fastened particles of air must contribute to the greatness of the effect; and in the last place, the difficulty either of detaching or extending them, must produce that of opening the valves.

It is certain, that to detach from the copper the parts of water which moisten it, there must be a pretty considerable effort, and it must be only by the evaporation, or by a violent friction, and at several essays, that we can entirely obtain the effect. As for the extension of the parts of water, neither M. *Amon tons*, nor all the rest who have been willing to make the experiment, have been able to affirm, that water is capable of extension. Thus we may believe, it is all reduced to the difficulty of detaching the parts of water. It is more than probable, that they do not fasten with the same force to all sorts of bodies.

It follows from the observation and reasoning of M. *Amon tons*, that in the experiment of the two surfaces, wetted and applied to each other in the open air, it is not only the pressure of the

atmosphere that makes their union, but the parts of water which wet them, have more or less share therein, according as they are more or less strongly fastened to the different surfaces of the bodies; and this is a cause which we did not think of in the explication of this *phenomenon*, tho' it was at least as important and efficacious, as that which was at first discovered, and it may act alone, or even combated by the other.

M. *Amontons* would have been glad to compare the force of the union produced by the parts of water, with that which is produced by the pressure of the atmosphere. The way was to immerge the pump deeper and deeper in the water, till at last the valves would open without difficulty; for we should have seen thereby what weight was exactly required to overcome all the resistance caused by the adhering of the parts of water, but this could not be put in practice, because of the particular circumstances of the place.

These observations on the mutual adhering of bodies, and on the forces which produce it, may serve one day to discover the general cause of hardness, which is none of the least questions in physics; but whilst we are expecting so elevated a theory, M. *Amontons* drew from his experiment something useful in practice. He changed the valves of metal for flaps of leather, after which the pumps played perfectly well, because leather not being near so solid as metal, the water does not adhere in the same manner. The inconvenience of the valves came from their being too well made, too well polished, and too well fitted upon their shells, and certainly we should never have expected, that too great a perfection would have rendered them useless. To prevent this accident, M. *Amontons* advises us to prefer leathern flaps.

XX. *On the recoil of fire-arms.*

M. *Geoffroy* having reported from the *philosophical transactions*, that it had been found in the *royal society of London*, that a certain charge of a fire-arm turned the ball from right to left, whilst the cannon in recoiling went from left to right; this fact, tho' absolutely possible, seemed very doubtful, and we were willing to be sure of it, before we sought for ingenious reasons, which we might perhaps have had the misfortune to find.

M. *Cassini* the son undertook the experiment, and he caused a machine to be made as like as possible to the *English* one. It was a wooden *Isosceles* triangle, upon which was placed and made very fast a gun, of about 3 feet 8 inches; the breech of it rested upon the base of the triangle, and the end upon the *vertex*. We must suppose, without entering into a more exact description, that the machine was sustained at its 3 angles upon 3 pivots, which hindered it from rubbing against the ground, that it might be rendered fixt and unmoveable by screws, which entered into its pivots, and that so by taking away the screws of the angle of the top, and of one of the angles of the base, it might turn upon the pivot of the third angle, as upon a centre, and that it turned very easily, because there were rollers at the 2 angles, which were desired to be moveable.

This being done, M. *Cassini* the son fastened the machine, charged his gun, fired it, observed the place where the ball struck upon a board placed at 17 feet distance, then he took away the screws from the angle of the *vertex*, and from one of the angles of the base, charged it again, and fired it.

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The machine must then recoil, because it was at liberty, and its recoil was the arch of a circle, which the moveable angle of the base described. The place was observed, where the ball had struck; the machine being at liberty, and it was compared with that of the *fixed* stroke.

The experiment was repeated a great number of times, and with several different charges, as well for the fixed strokes as for the others. As the machine always recoiled, or turned, when it was free, these strokes did not always hit the same place with the fixed stroke, but they always went to the right of the fixed stroke, because this circular recoil was made that way, and there never was found between the stroke and the recoil the contrariety of direction marked in the *English* experiment.

An advantage, which that of M. *Cassini* had, is, that every time he replaced his fixed machine, he was well assured, that it was found again in the same situation, in the same direction, and that it had received no alteration from the preceding shakes; for all the fixed strokes went always into the same hole. Perhaps, on the contrary, in the *English* experiment, the gun, which had not been held fast enough, turned from its first direction; and this might be sufficient to make the stroke have a contrary one to the recoil.

The recoil was always so much the greater, as the charge was stronger; but the strokes of the machine at liberty were not distant from the fixed points, in proportion to the greatness of the recoil. On the contrary, the stronger the charge was, and the greater the recoil, the less did the free strokes vary. This comes from the ball always going out with more velocity, there is less impression felt of the recoil, which does not begin
to

to shake the machine, till the ball is upon the point of going out.

XXI. *On the force of machines in general.*

The multiplication of forces by machines has something deceitful in it, not with regard to those who know the principles of mechanicks, but to the general part of the world, on whom the engineers may impose by magnificent promises; and sometimes to the engineers themselves, if they have more invention or boldness than knowledge.

The academy sometimes hears of some of these engineers and their lofty proposals. One man is to have as much effect as 50, to raise a burden, and when it is objected to them, that this man will therefore, according to the rules of mechanicks, travel 50 times more than the burden, and consequently spend much time, they do not always allow this necessary augmentation of the way and of the time; nay, they sometimes go so far, as to pretend that the rules of mechanicks are human inventions, which do not subject nature, and that we must believe experience rather than books.

This gave occasion to M. Amontons, in a paper which he read to the academy, to review the first principles of mechanicks, and to lay open the whole compass of them, with regard to the use of machines. We will here give the metaphysics of them, which are no less demonstrative than geometry, and may be more intelligible.

Whatsoever may be the cause of the resistance of bodies to motion, at least of those upon which we can act, it is certain, that they bring a certain resistance, and that it is the greater, as they are greater, or have a greater mass. And as a quicker motion, is a greater motion, this resistance is also

proportioned to the velocity, with which one would move them. Consequently, when a body is moved, the force which moves it is so much greater, as it has more mass and velocity; so that the force, or *quantity of motion* of a body, is a product of its mass by its velocity.

Of these two things, one is fixed, and always the same in all the motions which we can give to a body, and this is its mass; the other, which is its velocity, is infinitely variable in the different motions which may be given it, and there is no body, how small soever, that can go to such a degree of quantity of motion, or force, as we will, provided we give it a sufficient velocity.

Not only the same cause, which produces the resistance of the bodies to the motion, produces a greater resistance to a greater motion; but it does it only because it produces the resistance to the motion. These two effects therefore are but one; therefore, the resistance of a great body to a small motion, is the same with that of a little body to a great motion, provided the masses and velocities are proportioned, and provided the greater quickness belongs to the less mass. Therefore the forces necessary to surmount these resistances are equal, and in like manner the quantities of motion of two bodies moved in these conditions.

Thence it follows, that the same force or quantity of motion in general, may be formed in infinitely different ways, and all equivalent; for provided the product of the mass and of the velocity continues the same, these two magnitudes may vary infinitely between themselves. But if we consider a determinate body, to which we would give a certain quantity of motion, we cannot do it but by augmenting its velocity, seeing its mass cannot be changed.

No force can be *in equilibrio* but with an equal force, nor be surmounted but by a superior force. Thus all the art of mechanicks never renders a small force equal or superior to a greater; and every time that it seems, that a little force is *in equilibrio* with a greater, 25 *lb.* for example, with 100 *lb.* it is a sort of illusion upon the eyes, the *equilibrium* is not between 100 *lb.* and 25 *lb.* but between 100 *lb.* and 25 *lb.* so moved or disposed, as necessarily to move four times faster than the 100 *lb.* If we compare only these two fixed and immoveable magnitudes, 100 *lb.* and 25 *lb.* the 25 *lb.* seem to be multiplied and elevated above themselves, and this is the false marvellous in mechanicks; but it is dissipated by the 4 degrees of velocity, which were to be given to the 25 *lb.* and are a real and true force, tho' less sensible.

A force of 10 *lb.* moved 10 times faster than the 100 *lb.* would have equalled them in like manner, and it is the same with all the possible products equal to 100; but at last we must always find 100 *lb.* force, in whatsoever manner we take them, either upon the fixed, or upon the variable magnitude; this is an inviolable law prescribed by nature, which has left to art only the choice of the different combinations, which can make the same product of force.

A small power, therefore, not being able to move a great weight, if it does not make as much way in the same time as the weight will make little, there happen from it two insurmountable inconveniences; that the power, for instance, a man, is obliged to make a great motion, either of the arms or feet, for a long time, and that during this long time the weight, of which the elevation

vation is the object of the whole machine, is but little elevated.

It follows from this, that the time necessary for the power to make a certain way regulates the time, during which the weight shall be elevated or moved. For example, if a man having a force of 25 *lb.* raises a weight of 100, this weight cannot be raised a foot in less time than the man could go 4, and consequently it is from the man, or in general from the power that the time of the elevation of the weight takes the law.

Most engineers being dazzled, or willing to dazzle by an apparent multiplication of forces, either do not give attention enough to the time which it will necessarily cost them, or do not know how to calculate it with sufficient exactness; and this so essential a point not being cleared up, the machines are always wonderful as to the effect. Indeed, it is not easy to determine exactly what shall be the velocity either of men or horses, which are the powers most commonly employed; and it is for this reason, that M. *Amontons*, after having carefully tried their velocities in different actions, has given the following results of his experiments, which will furnish sure and commodious principles for the calculation of most machines that can be imagined, and even of several labours that are undergone without machines.

Expa-

Experiment

1. Two chairmen loaded, going their usual rate, made in 80"
2. A porter loaded, in 139"
3. A footman walking, in 120"
4. A footman running with his whole force, in 25"
5. A drawer of a loaded calash, in 86"
6. A horse drawing upon the pavement a cart loaded with 1500 *lb.* in 112"
7. Two horses drawing a coach upon the pavement, at the usual rate, in 62" } 70 toises.
8. Two other horses drawing a coach also upon the pavement, in a trot, in 45"
9. A saddle horse, with its rider mounted, going the common pace, in 80"
10. Another saddle horse, with its rider also mounted, going the *grand pas* in 50"
11. Some labourers carried each of them 22 baskets of earth in a day, each basket weighing 30 *lb.* to 370 toises distance, they came back empty, and this was certainly as much as they were able to do.
12. A man raised a weight of 25 *lb.* with a cord passing over a pulley, to the height of 220 feet in 145"
13. A man who weighed 133 *lb.* rose to the height of 10 toises 2 feet, on a ladder in 34", and was quite out of breath, and unable to proceed.

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14. Two horses fastened to a plough, in a soil neither too loose nor too stiff, made each of them an effort of 150 lb.

15. A sawyer gave 200 strokes of a saw, and raised it as many times in 145", at each stroke his hand went 18 inches, with an effort of 25 lb. he was very much out of breath, and could not go on above 3' without taking breath.

We see now in what limits of time a great number of common actions are contained, and if we would make use of others, it will be easy also to make a fundamental experiment, which will regulate the whole calculation of the time.

When we shall know the time, which a certain power will necessarily require, to move a certain weight, we shall find perhaps, that the slowness will be so great, that it will be better to increase the power, and to renounce the deceitful or dangerous advantage of employing but a very small one.

The whole art of mechanics in general consists therefore, not in multiplying the forces, but, according to the expression of M. *Amonçons*, which seems more proper, in *modifying* them, and employing them against other equal forces, in the manner that best agrees with the design. The difficulty of choosing well between the different manners is sometimes great enough to exercise the best *genius's*.

In particular, there are an infinite number of attentions to be made to the most advantageous application of the moving force, or, which is the same thing, to its strongest direction, to the means of hindering this direction from being changed in the actions which must be equal, or of making it, if it does change, to be recompensed elsewhere, to the frictions, which are all
put

put on the side of the weight against the power, which must consequently be avoided and diminished, as much as possible, to the situation of the centres of gravity or percussion, &c. Thus, when a machine answers its design well, there is no need to puff it up by a false multiplication of forces, to be sure of its being one of the finest works of human genius.

XXII. On frictions.

The new discovery of M. *Amontons*, that frictions are always proportioned to the pressure and to the velocity, and never to the surfaces, was important enough not to be received without a strict examination.

There are some cases, which seem evidently to prove the contrary. If, for example, two equal powers are applied to the extremities of a cord, which towards its middle is turned about a cylinder, and they draw one against the other; it is certain that one, in case it receives an augmentation, and becomes superior, cannot make the other come to it, but by making the cord move upon the cylinder, and by surmounting its friction; and it is certain also, that it will have need of so much greater augmentation, as the cord shall make more turns about the cylinder. Now, why does a greater number of turns of the cord about the cylinder render the motion more difficult, unless it is because the friction of the cord is made upon a greater quantity of parts of the cylinder, or upon a greater surface? for whatsoever number of turns the cord makes, the pressure caused by the two powers is always the same.

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M. *Amonions* answers to this, that it is always the pressure that renders the motion more difficult, and that it is so much the greater, as the number of the turns of the cord is greater, because we must reckon the same power as applied so many times to the cylinder, as the cord makes turns about it. Thus the power applied by a cord, which makes two turns, three turns, &c. becomes double and triple of itself. But as this multiplication of the same power might seem not very true and fit for the purpose, M. *Amonions* proves it much after the following manner.

Let the cylinder be cut according to its *axis*, into two equal parts; let one be suspended and immoveable, and the other be moveable and placed lower, and let it bear a weight fastened to it, and let a cord turn about both as often as you please, and let a power be applied to it, so that it may tend to make the lower half cylinder rise, loaded with its weight, we shall see that the power is in the same case, that if it tended to make the weight rise to the aid of a redoubled pully, and that consequently, if there is an *equilibrium*, it is to the weight, according to the principles of statics, as unity is to the number of half turns, which the cord makes about the cylinder; for unity is to this number, as the way, which the weight would make, to the way, which the power would make, in case the *equilibrium* ceased. All the effort therefore, all the action of the power consists in this power multiplied by the number of half turns of the cord.

Now let us suppose the lower half cylinder raised by the action of the power, its plane surface applied against the surface of the upper half cylinder, and the weight taken away, which would have endeavoured to separate them, all

the rest remaining the same, it plainly appears, that the power only presses one of these half cylinders against the other, that its action or effort are exactly the same as before, and that consequently they always consist in the product of this power, by the number of the half turns of the cord. Therefore independently on a greater surface of the two half cylinders being encompassed by the cord, and rubbed with it, the pressure of the same power is greater by the multiplication of the turns of the cord.

The pressure alone making by itself, according to M. *Amontons*, the whole resistance of the friction, the greater or less velocity, which we give to the bodies that rub, is a circumstance which augments or diminishes the effect of the pressure, that is, the difficulty of the motion. We must therefore observe to dispose machines in such a manner, that the parts which rub may have the least velocity possible. Thence M. *Amontons* concludes, that every time a wheel turns upon its *axis*, it must make the diameter of the *axis* the least that is possible with regard to that of the wheel; for the two surfaces of the wheel and of the *axis*, which necessarily rub, will have the less velocity, since the velocity of a circular motion always diminishes from the circumference towards the centre. For the same reason, when we make indented wheels, we must make the teeth as small, and as thin as possible. One of the teeth rubs by one of its surfaces, against an extent equal to the surface which rubs, and it must disengage itself in a certain time, by running thro' a space equal to the same surface. Therefore the smaller the surface is, the less space it has to run through; and it must here be observed, that the smallness of the surface diminishes the

resistance of the friction, not because it is a less surface that rubs, but because it is a less space run through.

Another observation of M. *Amontons* upon machines, with regard to friction, is, that we must be very careful that the line, according to which the moving force acts, may not be the same with that of the pressure; and the reason of it is quite natural, for the moving force would unite its action to that of the pressure, which is always contrary to it. This force must act as much as possible by the same line, according to which the bodies which rub are to move.

Notwithstanding all these proofs and observations of M. *Amontons*, who had set his system of frictions in a pretty good light, we are here obliged to acknowledge to the publick, that the academy was not fully persuaded of it. They allowed that the pressure was to be considered in frictions, and often to be solely considered, but they could not, with M. *Amontons*, absolutely exclude the consideration of the surfaces. He represented the great number of experiments, which he had made, where the surfaces went for nothing but experiments have a certain compass, more or less insensible, which may sometimes be so on occasions, where the determination of the question might have been made. Thus they were willing to carry the affair, even into metaphysics, and to seek what was to be thought of it in the first notions.

Let two bodies, having plain surfaces, be perfectly hard and polished, and let them be moved one upon the other; it is plain that the resistance caused by the friction, will either be none, or infinitely small; but if, instead of this hypothesis, which is not in nature, we conceive

two bodies, having rugged and uneven surfaces, the difficulty of moving one of them upon the other, can come only from the necessity of raising the first to disengage its parts let into the second, or from the pressing and rubbing of the parts of one against those of the other, or of both together. In the first case, the only difficulty of raising one of the bodies makes that of the motion, and consequently the friction comes only from the greatness of the weight to be raised, or, which is the same thing, from its pressure, and the greatness of the surfaces goes for nothing. In the second case, the greatness of the surfaces alone would do all, if it was possible that this second case was absolutely separated from the first, that is, that the parts of one body were rubbed against those of another, without raising either of them, for it is visible, that a greater number of parts to be rubbed make a greater resistance; but because we do not rub without raising, at least in practice, the resistance, which comes from the greatness of the surfaces, is always mixed in this second case with that which comes from the pressure; whereas in the first, that which comes from the pressure, may be single and without mixture. Besides that part of a body, which suffers a friction, is usually very small with regard to the great number of times, that it must be raised in the same friction, and to all the small heights taken together, to which it must have been lifted. Thus, besides that the resistance, which comes from the pressure, may be alone, besides its always accompanying that which comes from the greatness of the surfaces, it is usually much more considerable than that, when it does accompany it, wherefore in the greatest part of the experiments, it is the only

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only one that makes itself be perceived, and the only one that we ought to reckon. But as it is possible, that in certain cases the pressure may be very slight, and the number of parts to be rubbed very great, or what is to be rubbed may be very considerable, it must then happen, that the friction sensibly follows the proportion of the surfaces. This sort of metaphysicks, which we have just opened, may serve to give a more entire and perfect idea of the frictions, than that which might be drawn solely from experiments. When questions are of a nature to permit us to examine what ought to be, we may advance, that what ought to be well conceived, is also certainly that which is, and often sets right what seems to be.

XXIII. *On the course held by several bodies, tied together by cords, and drawn on a horizontal plane.*

Suppose several weights, such as you will, fastened to the same cord, so that the parts of the cord contained between these weights being stretched as much as they can be, may make angles between themselves, such also as we will. The cord being drawn by any force whatsoever on a horizontal plane every where equally rough and rugged, if all the weights are moved together, it is certain, that they will have different courses, from the directions, which they had before towards one another, which also were only the same parts of the cord where they are fastened. Now the question is to determine what all these different courses shall be, and the force necessary to move all these weights. This is what M. Parent has done, by a general method, which

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contains all the particular cases possible. We shall here give only the principles of them.

In the instant that the force, which draws a body on a rough horizontal plane, is ready to make it part, or which comes to the same, according to the geometry of infinites, in the instant that it makes it describe a space infinitely small, it is exactly equal to the resistance, which this body brings to the motion; that is, to its friction on a rough plane; for this is all that it has to surmount, in order to make it move. We shall here consider only the resistance caused by this friction, and the bodies drawn, only in the instant that they are ready to part, or describe already spaces infinitely small, and consequently are *in equilibrio* with the moving force.

If two weights fastened to the same cord are drawn by a force, the direction of which is in the same line with this cord, it is very plain, that provided this force is equal to the friction of the two weights, it will put them both in a state of parting, and moving according to the line of its direction. But if the direction of the moving force is oblique to the cord, which binds the two weights, then it must be considered, that the *first* weight, that is, the most distant from the moving force, resists the motion, which it endeavours to impress on the *second*, which is the nearest, and upon which we may conceive that it acts at first, that this second being drawn by the force, according to its direction, it is therefore at the same time in a manner withdrawn a contrary way by the first; that the direction of the first with regard to the second, is the very cord that binds them; that this cord being oblique to the direction of the moving force, the action by which the first weight resists the motion of the second,

second, or draws it back, is therefore oblique to the direction of the moving force, and that consequently according to the theory of compound motions, we must conceive this action of the first weight with regard to the moving force, as composed of 2 others, the one perpendicular to the direction of the force, the other parallel, or rather being in the same line with the direction of the prolonged force. This compound motion is the key of this whole matter. Seeing one of the two *simple* actions, in which the oblique action of the first weight with regard to the second is resolved or decomposed, is only the prolonged direction of the moving force, the first weight draws directly this way against the force, and consequently the force cannot move the second weight, without its being greater than the first. In this case, *M. Parent* retrenches from the direction of the force a part equal to the action of the first weight, or, which is the same thing, to its friction, and there remains a line, which represents the whole effort that the moving force can make according to its direction on the second weight. But the first had also a simple action, or that which is perpendicular to the direction of the force. Now by this action, it is not at all opposed to the motion which the force tends to impress on the second weight, for it might this way make the second move, without displacing the first, that is, the second would turn about the first, as about an immoveable centre, and consequently there would not be any friction of the first weight to surmount. The line therefore, which represents this single action of the first weight, remains entire; and lastly the diagonal of a parallelogram formed of this line, and of that which remains of the direction of the force, will represent the whole

whole action of the force on the second weight, modified by the resistance of the first, or, which is the same thing, the course which the second weight must follow. As for the first, as it does not draw any other weight after it, and as its motion is not modified by the resistance of any other, the direction of its motion, or the course which it takes, is the same line with the cord which binds it with the second weight.

The moving force therefore makes the two weights move according to two different directions; the first, according to the cord which binds it to the second; the second, according to the diagonal which we have explained, and consequently this force may be represented by these two different lines, and conceived as equal to their sum, with regard to the other actions or resistances represented by other lines.

The force and the two weights continuing the same, if the direction of the force is supposed more oblique to the cord of the two weights, the single action or resistance by which the first weight draws the second back directly against the force, becomes the greater, and it may become so to such a point, that the force will be no longer capable of moving the second weight this way, but only of making it turn about the first, which will be immoveable. The *equilibrium* therefore here, as usual, depends on the greatness and direction of the weights or forces.

If the force was not great enough to overcome the friction of the second weight, it could never move the first, which cannot absolutely proceed without the second.

Now, if instead of two weights, we suppose three bound by cords, which make angles between themselves, we must make upon the the third and

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second the same reasonings, which *M. Parent* has made upon the second and first. The second will draw back the third in a contrary way to that of which the force will draw it; and because the direction of the second is supposed oblique to that by which the force draws the third, this third will describe a diagonal, which will be found as that of the second was, when there were but two. But it must be observed, that in this case, where there are three, the course or diagonal of the second is no longer the same that it was, and the reason of it is, that the force draws it no longer according to the same direction as before, since we suppose it draws it by an oblique direction to the cord which binds the second and third. Now it is visible, that the direction, by which the second weight is drawn, cannot change without its course changing. This alteration of the course of the second weight, when there are three, has escaped a geometrician of the first rank, who has examined the same case, and *M. Parent* thinks himself the first, who perceived this slight inadvertency.

The case of the three weights being well understood, a greater number of weights does but render the application of the principles a little more complicated and difficult. The course of the bodies being found, the value of the force which makes them describe these courses, is soon found; it is nothing but a calculation, which we leave to the algebraists, and it is sufficient for us to have given the soul of it.

A N ABRIDGMENT OF THE Philosophical Memoirs

OF THE

ROYAL ACADEMY of SCIENCES at *Paris*,
for the year 1703.

I. *Observations on the quantity of rain that fell at Paris at the royal observatory, on the thermometer, and on the barometer, during the year 1702, by M. de la Hire **.

THE year 1702 has been looked upon as one of the most extraordinary that have been for a long time, for the drought of the spring and summer. Yet it has been one of the most fruitful in grain, excepting the summer corn, which may be ascribed to the natural coolness and moisture of the lands of this country. It is also observed, that when the end of spring and beginning of summer are rainy, the weeds grow very much, and turn up the corn; and besides in moist seasons there often come fogs, which spoil the grain when it is beginning to form.

The quantity of water which fell in rain, or in snow, was in the months of

	<i>Lines.</i>		<i>Lines</i>
Jan.	18 $\frac{1}{4}$	July	19
Feb.	18	Aug.	35 $\frac{1}{4}$
Mar.	9 $\frac{1}{2}$	Sept.	11 $\frac{1}{4}$
Apr.	17 $\frac{1}{2}$	Octob.	15 $\frac{1}{2}$
May	5 $\frac{1}{4}$	Nov.	18
June	9	Dec.	18

* Jan. 10, 1703.

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And

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And

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And so during the whole year the sum was 196 lines, or 16 inches $\frac{1}{2}$, which is much less than in the common years, which give 19 inches of water. But it must be observed, that very often the 3 months of *June*, *July*, and *Aug.* furnish as much as all the rest of the year, which did not happen this year, since these months were not different from the rest. And tho' the water, which falls in these 3 months, is in great abundance, which comes only from some storms that go over quickly, it contributes but little to the nourishment of plants, as it is suddenly raised in vapours by the great heat of the earth and air, or runs off into the ponds and brooks, without penetrating far into the earth.

My thermometer is always situated in an uncovered place, where neither the sun nor wind can affect it, and is filled with spirit of wine coloured, and is sealed hermetically.

It was at the lowest this year at 14 degrees $\frac{1}{2}$ *Jan.* 1. which was a sign of very great cold; but from the 6th it rose to 43 degrees, and it kept up all the rest of that month, and even at the beginning of *February* to about 40 degrees, sometimes a little more, and sometimes a little less; tho' the cold is usually greater at the beginning of *Feb.* On *Dec.* 25. it was at 44 degrees $\frac{1}{4}$, which is within about 2 degrees as high as it was in the beginning of *June*. On *Dec.* 17. it was the lowest since the beginning of winter at 30 $\frac{1}{3}$.

June 19, it was at 60 degrees $\frac{1}{2}$, *July* 29, at 61, and *Aug.* 6, at 62, which is the highest it came to; but *Sept.* 2. it was again at 61 $\frac{2}{3}$. Whence we know in general, that during the months of *June*, *July*, *Aug.* and the beginning of *Sept.* there were very great heats. The greatest height of the liquor of the thermometer was
about

about 2 in the afternoon at 71 degrees $\frac{1}{2}$. Aug. 5. There fell no snow, or but very little, either at the beginning or end of this year.

The quicksilver rose in the tube of my barometer to 28 inches, 2 lines $\frac{1}{3}$, Feb. 11. The wind was then S. as it was for some days before and after, tho' the barometer always kept to about 28 inches, which does not agree with what we usually observe of the height of the quicksilver in the barometer and of the wind. It was at the lowest 26 inches, 5 lines Dec. 20. with a moderate S. wind, which is very extraordinary; for this great falling of the quicksilver seemed to indicate some great storm. The difference therefore between the highest and the lowest, was 1 inch, 9 lines $\frac{1}{3}$, much more than usual.

M. *Amontons* having proposed this year to the academy a thermometer, like that of *Sanctorius*, which is not sealed, and having the degrees of heat marked by the elevation of the quicksilver in a tube, instead of *aqua fortis* that had been used in engraving, which *Sanctorius* employed, the heat and cold of the exterior air acting on that of the phial of the thermometer, and having observed that the boiling water, in which he had plunged this phial, could cause but a certain dilatation of the air inclosed therein, let the fire be ever so much augmented to make the water boil, he judged to be a mean of having a term known thro' all the world, to which the different degrees of dilatation or heat of the air might be referred; and having given me one of these thermometers, filled after the manner explained by him to the academy, I exposed it to the strong summer sun behind a glass, about noon, in a place close shut up, and I observed that the quicksilver rose in it at the highest to 31 inches, 5 lines, about 2 in

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the afternoon, *Aug. 5.* The quicksilver of my barometer was then at 27 inches, 10 lines $\frac{1}{3}$.

The declination of the needle was *Sept. 22, 1702*, $8^{\circ} 48'$ towards the W. It was observed in the same place, as in the preceding years, and with the same needle, 8 inches long.

II. *The thermometer reduced to a fixed and certain measure, and the means of referring to it the observations made with the old thermometer, by M. Amontons *; translated by Mr. Chambers.*

We are all agreed, that heat is the general cause of all the effects, and all the productions upon our earth; and that without this, the whole would be a fixed motionless mass. Heat, therefore, being as it were the soul of nature, 'tis of the utmost importance to philosophers, to be able to measure it with exactness; so that the instruments which serve to determine the several degrees thereof, cannot fail of being cultivated in a science, whose whole object is the knowledge of such effects and productions.

But as this knowledge is not the work of a day, but rather of many ages; and as there is no arriving at it, but by a long series of observations, the application whereof frequently cannot be found till long after they have been made; one of our chief cares should be to preserve and transmit them to posterity, who may reap the fruit of their fathers pains; thus, for instance, it is, that by the length of the pendulum, which vibrates seconds, we have already ascertain'd all such as depend upon measure; and thus by the

* April 18, 1703.

equilibrium, which we have found means to make between the atmosphere and other fluids, whose weight is known, we may perhaps be able to ascertain all such as depend on the air wherein we live, which, according to many, contains the first principal of life, and which therefore cannot be too much cultivated.

Sanctorius, in his commentary on *Avicenna*, has left us several methods, whereby he apprehends this might be attain'd; but the thermometer he gives us, acting at least as much by the weight or lightness of the air, as by its heat or cold, the common spirit of wine thermometers are justly preferr'd thereto; and for the like reason, the thermometer lately invented by me, is preferable to those others; I may add, that being free from all the defects of the former thermometers, there would be nothing further to be desir'd on this head, were it but easily portable, and no necessity in using it to rectify it by the weight of the air.

But as the utmost care is to be taken, not to invert it, which would intirely discompose it; and as in every time of observation, regard must be had to the greater or less weight of the atmosphere, in order to allow for what it exceeds or comes short of 28 inches; it will not suit all kinds of persons. Upon the whole, its chief use is, for perpetuating to future ages the observations made by our thermometers, and to serve as a standard, whereby to regulate all thermometers which have the same scale. The manner of effecting which, we proceed now to shew.

To regulate a spirit of wine thermometer by an air thermometer.

* As to the figure of the glass, and the liquor contained in it, the new spirit thermometer does not differ from the common ones, excepting its magnitude and scale, which is exactly equal to the scale of an air thermometer, when rectified by the weight of the air; its graduation is by inches and lines, of which there are two orders, one ascending, the other descending.

The ascending scale shews the degree of heat, that is, the quantity of inches and lines height of mercury, which the heat enables the spring of the air to sustain; from whence it follows, that the extreme degree of cold in this thermometer, would be that which should reduce the air, not to sustain any weight of mercury by its spring, which must be a degree of cold much more intense, than what we commonly call extreme cold, since by experiment it appears, that if the heat of boiling water enables the spring of the air to sustain a weight equal to that of 73 inches of mercury, the degree of heat which remains in the air, when water freezes, is sufficient to enable it to sustain 51 inches $\frac{1}{2}$, which deserves particular notice.

The graduation which descends, shews the degrees of cold below the heat of boiling water, that is, the number of inches and lines, whereby the diminution of heat, below that of boiling water, enables the air to sustain less mercury.

Now these degrees of heat and cold, indicated at the same time, by the new thermometer, are always the complements of each other to 73, which shews, that when one is known, the other is likewise known, the whole as represented by the adjoin-

* Plate I. Fig. 2.

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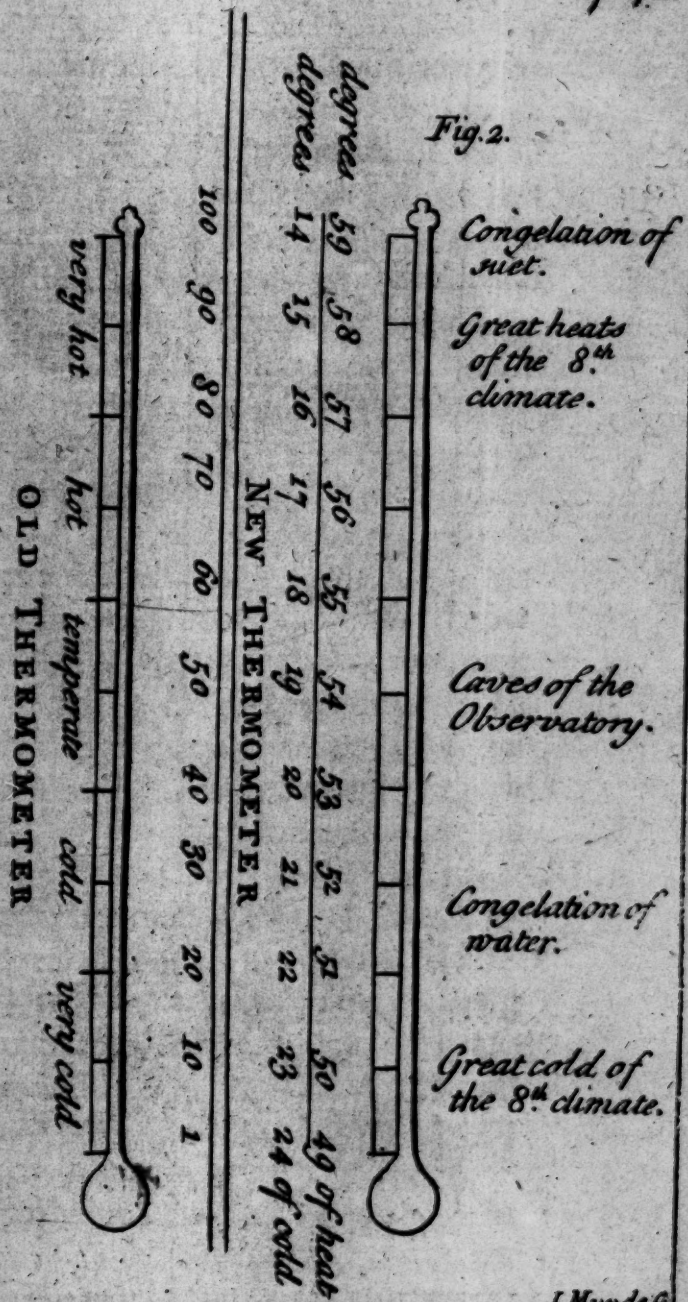
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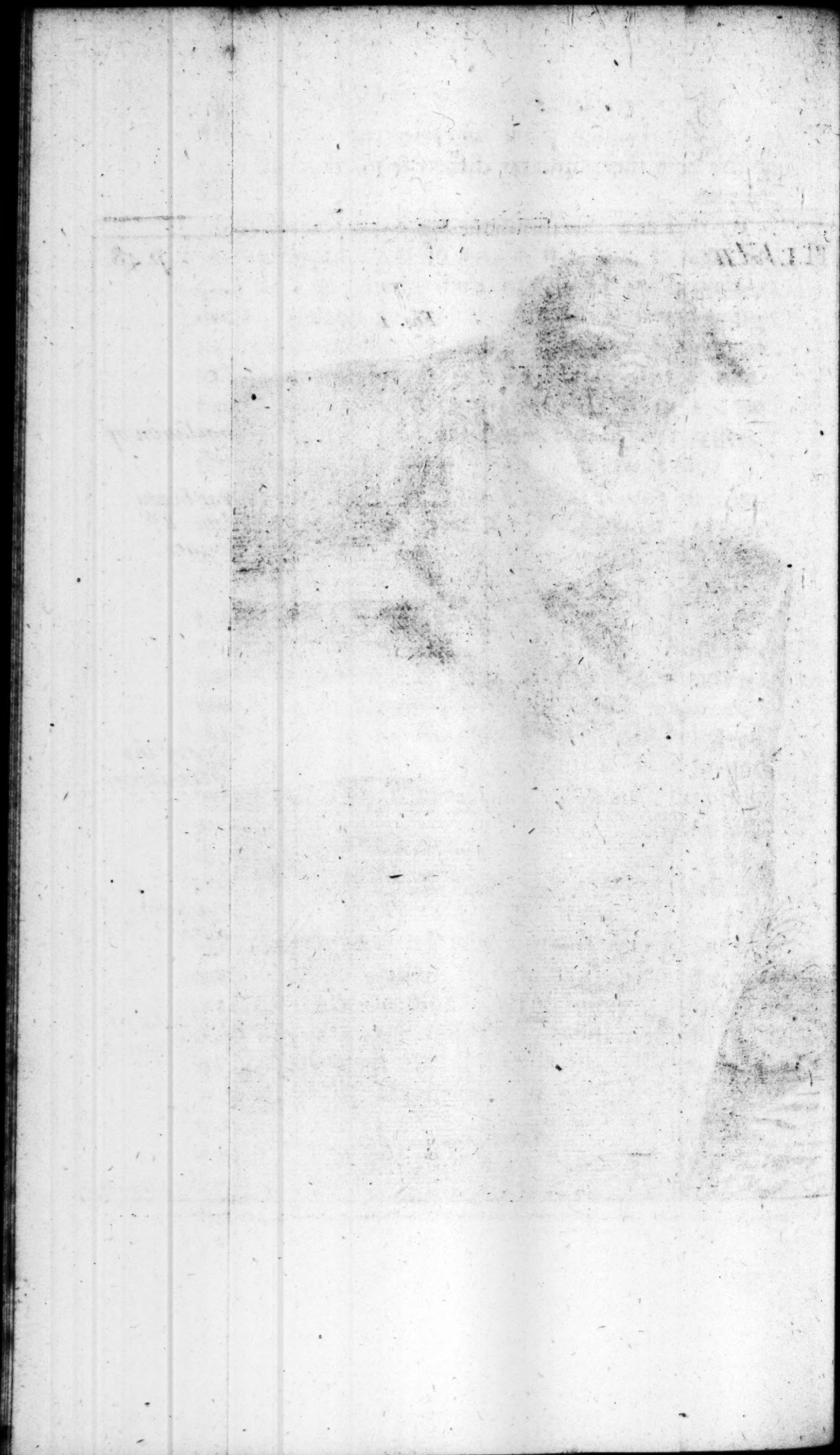


Fig. 1.



Fig. 2.





ing figures, which shew wherein the graduation of the new thermometer differs from that of the old one.

By this new thermometer we have found, that the greatest and least degree of heat, experienced at *Paris*, are nearly to each other, as 6 to 5; so that between the greatest heat of summer, and the severest cold of winter, the difference is only about a 6th part. Hence, as in our winter's cold, a great part of the fluid bodies lose their fluidity, 'tis highly probable, that in an absolute cold, wherein there were no remains of heat, no fluid body would be excepted from congelation, which seems to prove, that solidity is the natural state of all bodies.

Tho' the thermometers which now obtain, will at best only serve to make a gross comparison of the degrees of heat, without measuring them, yet, as multitudes of observations have been made therewith, that posterity may not be altogether deprived of the benefit of them, we shall subjoin a method of rectifying them, by comparing the thermometers used with that here described.

To rectify observations made with the old thermometers by the new one.

Place a new thermometer for some days, aside that which has been used in making observations, and after an hour or two, note at what degrees both of them stand; in a few days you will find them considerably alter'd; note their degrees as before, then divide the whole space, which the old thermometer has moved, into as many parts, as the space moved by the new one contains lines, and by these parts make a new graduation aside of the old one, distinguishing them

by 12 and 12, in the same order, and mark'd by the same figures as in the new thermometers, with which they will perfectly agree, excepting that they will be greater or less, as those thermometers are more or less sensible.

To distinguish these divisions from the rest, they may be called reduced lines, 12 whereof will make the reduced inch, so that the two thermometers will at the same time point out the same inches and lines, that is, the same degrees of heat and cold; this done, it will be easy to reduce the degrees of any former observations, into degrees of heat or cold, whose effect is better known.

Suppose, for instance, it were required to know what the degree of heat was, when the old thermometers stood at the 50th division of their scale? after making a new scale, as already directed, we shall find that the place, corresponding to such 50th division, will be at 54 inches on the ascending, and 19 on the descending scale, which shews, that the degree of heat, which raised the old thermometer to the 50th division, is the same which enables the air, in the ball of the new thermometer, to sustain 54 inches of mercury, and that this degree is about $\frac{3}{4}$ of that of boiling water.

III. *Remarks on rain water, and on the origin of springs; with some particularities concerning the construction of cisterns, by M. de la Hire *.*

All that concerns water, both for the necessities of life, and the ornament of palaces and gardens,

* April 18, 1703.

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has always been regarded as one of the principal forts of knowledge, necessary for men. The greatest care has been taken to render the very smallest rivers capable of carrying great boats, and by this means to join very distant seas together. Very plentiful springs have been brought by long turnings, and upon very elevated aqueducts, even to places where nature had refused to supply any. In short, a great number of machines have been invented, to raise water, and to carry it even to the top of mountains, to distribute it afterwards under a thousand different figures, with supernatural motions, and to make a sight of it, worthy of admiration. This was enough for people in general, but the curiosity of those, who inquire into the secrets of nature, was not enough satisfied; they wanted to know the origin of these so abundant sources of water, which are found all over the earth, and even upon very high rocks, and this is what has so much exercised the ancient and modern philosophers.

We find two principal opinions concerning the origin of springs, each of which is founded upon experiments, which seem indubitable; for it is evident, that several springs have for their principle the rain water, and snow melted upon the mountains; but how can these rains and these snows, which are very rare upon steep and high rocks, and in very hot countries, furnish the very plentiful and lasting springs, which we see in several places.

The rest of this paper is translated by Mr. Chambers.

But the great objection is, that we have springs even upon high and steep rocks, and in the hottest countries, where little rain or snow is known

to fall. Others, therefore, chuse to have recourse to certain subterraneous cavities in form of alembicks, wherein the vapours, issuing from the sub-jacent sea-water, and rising thro' the clefts and crannies of rocks, are condens'd by the cold of the surface of the earth.

M. *Mariotte*, to support the former, made an exact estimate of the quantity of rain and snow, which falls on the tract of land that furnishes the water of the river *Seine*, and found, that much more fell than was necessary to maintain the river at a moderate height, during the course of a year.

Upon reading the treatise of Dr. *Plott*, *De Origine fontium*, printed 1685, I was led to make a like examination myself of the quantity of water, which the rain and snow may furnish towards the formation and maintenance of springs and rivers; and I began with enquiring, what quantity of rain-water falls on the ground in the compass of a year; the result whereof I have yearly communicated in the memoirs of the academy, by which it appears that the depth of water, which falls at the royal observatory, *communibus annis*, amounts to between 19 and 20 inches, which agrees pretty nearly with what M. *Mariotte* had found.

But being in doubt, whether this whole quantity of water might be supposed to go to the formation of springs, I made the following experiments to satisfy myself hereof——chusing a place in the lower terras of the observatory, I planted a leaden bason therein eight feet under ground, the bason was four feet in surface, and six inches deep, being a little inclin'd towards one of its angles, to which I had solder'd a leaden pipe 12 feet long, one end whereof, after a considerable

able descent, came into a cellar; the bason was contrived at a distance from the cellar-wall, that it might be surrounded with a greater quantity of earth, and might not be kept dry by the proximity of the wall; close by the orifice of the pipe in the bason I put several flints of different sizes, to prevent the orifice from being stopp'd, when the earth should be thrown over it to the depth of eight feet. The soil is of a middle nature, between sand and loam, so that the water might easily penetrate it, and its upper surface is quite level.

Now I apprehended, that if the rain and snow water penetrate the earth, till they meet a clay or compact earth to stop them, the bason must here have the same effect, so that I might expect to have a spring of water bursting into the cellar.

But having no great opinion that this would succeed, I placed another bason at the same time, only eight inches under ground, the surface of which last was 64 inches, and its side or rim eight inches high; for this purpose I had chose a place screen'd both from wind and sun, and took care to pull up all the grass and other herbs which grew over the bason, that all the water, which should fall on the ground, might pass uninterrupted to the bottom of the bason, wherein was a little hole, with a tube to convey the water into another vessel.

The effect was, that in all the space from the 12th of *June* to the 19th of *Feb.* following, no water came by the tube from the bason; and tho' it began to run now, this was intirely owing to the great quantity of snow which had fallen, and was now melting. From that time the earth in the bason was always very moist, tho' the water would only run a few hours after raining, and

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it ceased running, when the quantity fallen was drained off; for some always remained in the earth, and would not go off, unless some new rain chanced to fall.

A year after, I repeated the same experiment, but only buried the bason 16 inches under ground, which was double its former depth. I took care likewise, that there were no grafs on the ground, and that it might be screened from the sun and wind, which would dry it too fast. The effect was much the same as in the former, excepting only that when a considerable time passed without raining, the earth would grow a little dry; so that a moderate rain coming on, it would not moisten it sufficiently to make it run.

Lastly, I planted herbs on the ground over the bason, but found, that when these were grown up a little, the ground was so far from sending me any water after rain, that all that fell was not sufficient to sustain them, but they would droop and wither, unless sprinkled from time to time with water.

Upon this, I bethought myself to measure the quantity evaporated thro' the leaves of plants, when exposed to the sun and wind, and accordingly on the 30th of *June*, at $\frac{1}{4}$ an hour past 5 in the morning, I put a pound of water, weighed very exactly, in a glass phial, with a narrow neck, and gathering 2 leaves of a fig-tree, of a moderate size, which together weighed 5 drachms, 48 grains, I immersed the ends of their foot stalks in the phial of water. These leaves when gathered, were very fresh and firm; but after exposing them with the phial to the sun, which was very hot, and in a place where there was but little wind, and stopping the neck of the phial left vacant by the foot-stalks very exactly with paper,

per, that no water might evaporate this way, I weighed the whole again at 11 the same morning, and found, that there had been a consumption of 2 drachms, which the sun and air had drawn away from the moisture of the leaves, and which can only be repaired, when the leaf is growing to its tree, by the moisture of the earth convey'd to it thro' the roots.

I also made several other experiments upon plants, and always found a great diminution of their moisture; in effect, having measured the surface of some leaves, and considered the quantity of them, which usually covered the ground, I apprehended, that the rain was by no means sufficient, especially in spring, to support them, without some foreign assistance. 'Tis true, the air and dew of the night supply the larger trees, and even plants, with a great stock of moisture, which we commonly find on the leaves about the sun-rise, and which descended into the roots, may help to support them part of the day; but this moisture alone would by no means suffice, were they not to borrow a great quantity from the earth, and the rain which pierces the same, as may appear from the following experiments.

What has been said abundantly shews, that the rain which falls on the ground where there is grass, or other plants, cannot penetrate it 2 feet deep, excepting, perhaps, in gravelly or stony places, where it may sink with more ease. Of this, we have an instance in the rock of *St. Baulm* in *Provence*, where the rain falling on this rock, which is all cleft and chinky, as well as void of grass and other plants, penetrates in a few hours time into a grotto, 67 fathom below the surface of the rock, and forms a beautiful cistern. Hence on such rocks, and in large solutions of snow in
sum-

summer, we frequently find certain springs overflow at a great rate, during certain hours of the day, and this even several times in a day, in case the sun only shine on the snow in certain different parts of the day, the points of the rocks, &c. keeping it in the shade all the rest. This we take to be the reason of those springs in the middle of the land, which have an ebbing and flowing like the sea.

These experiments convinced me, that I was not to expect the rain or snow water to pass thro' 8 feet of ground into the first basin, which I had lodged for that purpose; accordingly in the 15 years that it continued there, it never yielded a single drop.

Whence it appears, that 'tis but few springs can owe their origin to rain and snow; so that we must of necessity have recourse to other causes, in order to account for the formation of such plenteous and perenneal springs, as we frequently find in high places, and at a very small depth under ground. For instance, that of *Rungis* near *Paris*, which, however, is equally inexplicable, from the supposition of those subterraneous alembicks, or grottos, which serve to collect and condense the vapours of the sea water; for there are no rocks in the country all round, as I have learnt by several wells, which I have ordered to be dug, and that the ground is but little raised, and the water very near the surface. This spring supplies 50 inches of water in a continual stream, without any sensible change or interruption, and yet the whole space of ground which it can arise from, would not afford so much water, were all the rain that falls on it collected and none spent any other way; whereas the ground is all tilled, and covered either with grass or corn; add, that in

some vallies near this place, they are forced to dig very deep ere they come at water. Others have endeavoured to account for these springs, by supposing certain natural canals to convey water from some high river, and which passing over high and low places, and even perhaps under rivers which come a-crofs them, are so close and well lined, as not to let their water escape by the way, but bring it into the place where it bursts out of the ground: but admitting the reality of such subterraneous canals, I am satisfied, they could only have a descent necessary to make the water flow upon a clay bottom; but to suppose natural tubes high and low still corresponding, is as much as art can compass in a little garden.

Yet one further objection lies against this hypothesis; for if these high springs arise from rivers, those rivers must derive their water from other springs still higher; for those formed by rains and melted snow in places where the bottom is firm, can only form brooks or torrents, which will be of short continuance, and by no means able to supply the continual flux of such rivers. The large collections of water, as ponds frequently found at the head of small rivers, prove nothing as to the origin of rivers; since, by several experiments we have found, that more water exhales from a wide vessel, than can fall into it by rain.

There only remains one method of accounting for the origin of these springs; and this too is liable to some difficulties. ——— We are to suppose then a great quantity of vapours, to pass thro' the earth, being raised from funds of water usually found therein, about the level of the sea, or the nearest rivers; that these waters pass the more freely, as they meet with a soil easier to be

penetrated; something of which we find upon opening deep pits in the winter. Now the particles of such vapours may be united again and condensed into water, either by the coldness of the surface of the earth, as they approach thereto, or by meeting with a soil already soaked with water, or by some other matter which may be disposed to fix them, as we find that salts exposed in the air catch, and retain the watery particles floating therein. Upon this the water growing still in quantity, and coming at length to meet with a bottom firm enough to sustain it, glides along the same, till at length it either breaks out on the surface of the earth, where its firm bottom happens to terminate, or sinks lower into the ground, in case it meets with apertures in the clay bottom. — This seems to me the most probable account; and yet in this we are to conceive, that the vapours have particular canals whereby they pass, and which are so close, that the water formed of them cannot escape through the same.

Being desirous to know by experiment, what might be expected from the manner of condensing the vapours by their fastening upon stones, replenished with salts, I put a glass vessel in one of the cellars, at the bottom of the quarry of the observatory, and on the edge of the vessel fastened a piece of linnen, which I had dipped in water, wherein salt of tartar had been dissolved. This salt I chose, as apprehending it more disposed to fix the vapours than any other; add that the place appeared very moist, especially in summer. Some time after, I found the bottom of the vessel filled with a liquor, which was only the water produced by the vapours in the air, which gathering upon the linnen, till it was
fatu-

saturated therewith, the surplus had trickled down along the sides of the vessel. This experiment I would have carried further, to see whether the liquor would have continued running, and whether the salt in the linnen were carried off by the water; tho' 'tis possible, that stones, containing salts fit to fix the vapours, might still preserve their salt, and even acquire new: but, in my absence, some people coming into the cellar, broke the vessel, and put an end to the experiment.

I shall not here insist on some extraordinary kinds of springs, found by the brink of the sea, and upon high rocks; which ebb and flow like the sea itself, and yet afford fresh water. This I have elsewhere accounted for mechanically, by supposing subterraneous reservoirs, a little above the level of the sea, and that the cavity wherein they are placed, communicates with the sea by certain canals.—For it must follow, that when the sea rises, it will compress the air in these cavities, which, pressing upon the water in the reservoir, will oblige it to issue, and even arise thro' certain clefts or passages of the rocks, till it arrive at the surface of the earth, where it will form a spring, which must gradually diminish, as the sea ebbs or retires; that is, as the air, before compressed, returns to its natural state.

Thus much for the origin of fountains: I shall now proceed to some observations, which I have made on this occasion, as to the advantages which may be drawn from rain water.—The most considerable is, that of collecting it in subterraneous reservoirs, called cisterns; where, after it has been purified by passing thro' river sand, it will keep many years without spoiling. This water is usually the best that can be used, either for

drinking, or for the other œconomical purposes, as washing, bleaching, dying, &c. by reason it is not mixed with any salt of the earth, as all spring waters generally are, such cisterns will be of great advantage in places where there is no spring water, or even where all the well waters are naught. I need not here enter upon the construction of such cisterns, nor the choice of the materials to be used therein, since all that is required is to have a place which will hold water, and that the stones and the mortar they are joined with, may not communicate any vicious quality to the water, which is to continue there a considerable time.

Those who have such cisterns, and are curious of their water, take care not to let any water of melted snow, or even of stormy rains, enter the cistern. The former, I think, they have some reason to exclude, not on account of any salts, which they imagine mixed with the particles of snow, but only because such snows usually lie several days, and sometimes whole months on the roofs of houses, where they are spoiled and corrupted by the dung of birds and other animals, as well as by their long continuance upon tiles and slates, which are always very dirty; for this reason, when it begins to rain, I would have the first water which falls from the roof, be thrown away as naught, having only served to wash the roof, which is always covered with dust, raised from the mire dried in the streets and roads, and only admit that which comes afterwards into the cistern.

The following observation relating to the water which is to be excluded from cisterns, I fell upon accidentally, having been engaged for some years in collecting the water which falls at the observatory,

servatory, by means of an iron bason tinn'd over as already described in another paper; I used to wash the bason as well as the vessel, into which it conveys its water, as quickly as possible, at the beginning of any rain, which promised to be considerable, and put up the water as it came into clean glass bottles, to preserve it; but being desirous at length to taste this water, I was surpris'd to find it very ill savoured, and that it had a cast of smoke, which to me appeared very extraordinary, as I had often tasted other water gather'd in the same way, which had nothing of this taste, nor could I see any thing that might give it this smoaky taste; the place where it was collected being very open and high, and no chimney near it; but considering at length that this water fell with a north wind, which is not very common, that being a wind when it rarely rains, and that the whole city lies to the north of the observatory, the smoke of the chimnies might easily have mixed with the rain; and thus become collected along with it, which would naturally give it the taste abovementioned; but of this I was further assured sometime after, for gathering some rain-water which fell with a south or south-west wind, on which quarter there is nothing but open fields, I found no signs in it either of the smoaky taste or smell, from whence it follows, that all those rains are also to be kept out of the cistern, which are brought by winds which pass over places infected with any ill smell, as sinks, lay-stalls, large cities, &c. for that a little water, impregnated with any noisome vapours and exhalations, coming to mix in the cistern, will corrupt all the rest.

In effect, it being evident from all experiments, that rain water purified in river sand, of
all

all its mud and earthy smell, is both the most wholesome and useful of all others; I have thought of the most convenient method of preparing cisterns, to furnish every house with a sufficient quantity of water—in order to this, it is first observable, that a common house about 40 fathoms in surface, if properly roofed over, will gather every year about 2160 cubic feet of water, reckoning only 18 inches *per annum*, which is the least I have known fall. Now these 2160 cubic feet, will be equivalent to 75600 pints of water, at the rate of 35 pints to a foot, which is the just proportion for the *Paris* pint; dividing therefore this number of pints by the 365 days of the year, we shall have 200 pints a day; so that supposing 25 persons to live in the house abovementioned, there will be eight pints of water *per day* for each, which is more than sufficient for all the uses of life.

It remains only to give some instructions, relating to the place and manner of building such cisterns in private houses: in several towns of *Flanders*, near the sea-coast, where the well-waters are all brackish and bitter, by reason the soil is only a light sand, which the sea-water can pass thro' without being purified and sweetened; every house has its several cisterns, but these cisterns are under ground, and are only a sort of cellars, where they imagine water will keep better than in the open air; and in effect, thus much is true, that water, especially that of rain, will not keep in the air, by reason of the mud it abounds with, which it has not wholly deposited and left behind it in passing thro' the sand, so that it will corrupt, and a greenish kind of moss grow over its surface; for this reason I would recommend a place to be

be contrived in every house, about six feet above the level of the rest, eight or 10 feet high, and only a 40th or 50th part of the surface of the earth, which in our instance would amount to about a fathom. This place being built round with thick walls, a leaden cistern should be lodged in it, into which all the rain water should be convey'd after passing thro' a proper quantity of sand; a very small door would suffice for it, which being very thick withal, and matted over, would hinder the frost from reaching the water; and from such a conduit might the water be easily distributed into the kitchens, &c. This water being well screened, would keep as well as if under ground; and its being a little elevated above the level of the ground, would serve for its commodious distribution to all the lower parts of the house.

I have lately examin'd the several rain waters which I had formerly collected, and kept in glass bottles, and find, that tho' some of them have a very ill taste, which might be owing originally to the smoke, or other circumstances above mentioned; yet others remained perfectly good and pleasant, without any thing of that earthy taste found in all rain waters. I suppose this is owing to their having settled and deposited their mud, which we commonly find at the bottom of vessels, where rain water has stood any time.

I shall close with one further remark, which I made on some spring water, near *Montmartre*; this is a clear and good water for drinking, yet if flesh and the common potherbs be boiled with it, the broth will be extremely bitter, which cannot be owing to the nature of the herbs of the place, since if rain water be made use of, the broth will not have the least cast of bitterness.

IV. *That the new experiments on the weight and elasticity of the air shew, that a moderate degree of heat, may reduce the air into a sufficiently violent state, to be the sole cause of very great earthquakes and commotions on the terrestrial globe, by M. Amontons *; translated by Mr. Chambers.*

This surprising paradox is wholly founded on this, that we know no bounds of the condensation of the air, any more than of its dilatation; and that this peculiar property it has of being reducible by pressure, into bulks reciprocally proportional to the weights it is compressed withal, may make it surpass many times the weight even of the heaviest bodies, and increase the force of its spring accordingly, in which state it acts upon it with great violence; for tho' it has been affirm'd by Dr. *Halley*, in the philosophical transactions, that according to the experiments made in *London*, and in the academy *del Cimento*, no power is capable of reducing air into a bulk 800 times less than what it possesses on the surface of the earth. As he does not relate those experiments, which must needs be very difficult to make with exactness, and wherein of consequence it will be easy to be deceiv'd; and as on the contrary the experiments I have made, convince me, that the spring of the air, consisting only in the motion of the fiery particles among which it floats, and wherewith it is continually pervaded, it does not appear that we can expel them entirely by any force whatever, nor consequently reduce the air to an inca-

* May 19, 1703.

capacity of further condensation; for 'tis evident, while any matter remains between its particles, so fluid and moveable as we conceive the corpuscles of fire to be, nothing can hinder this condensation of the air from continually increasing more and more, provided the cause which produces it be increased accordingly.

Be this as it will, as we owe a great deal of deference to the accuracy of the great men who have made these experiments, we do not pretend intirely to disallow them, only it were to be wished, that experiments of this importance were more known than they are; in the mean time, till we have an occasion of informing ourselves hereof, or of making experiments of the like tenor ourselves, we shall suppose any bounds of the air's condensation as well as dilatation unknown, nor make any scruple for stretching them as far as we may have occasion for, with this restriction, that our reasoning shall be reduced to the terms of the experiment; whenever it appears certain and decisive.

According to the experiment of *M. de la Hire*, related in *M. du Hamel's* history of the academy for the year 1696, a column of air, 37 fathoms 3 feet high, over the surface of the earth, only weighs as much as 3 lines $\frac{1}{6}$ of mercury, when pressed with the weight of the atmosphere, which was then 27 inches 5 lines $\frac{2}{6}$; but as this weight for diverse reasons does not always remain the same, but is sometimes less and sometimes more, we shall suppose, for the more easy calculating, that a column of air, 36 fathoms high, pressed with 28 inches of mercury, weighs as much as 3 lines thereof, which amounts nearly to the same thing, tho' it comes nearer the truth, as will appear hereafter.

Suppose then with M. *de la Hire*, after the experiments of M. *Mariotte*, which we ourselves have verified, that quantities of air, equal in weight, possess places, which are reciprocally proportional to the weights wherewith they are pressed; on this principal, the weight of the air, which would fill the whole space compressed by the terrestrial globe, would be equal to a cylinder of mercury, whose base is equal to the surface of the earth, and whose height contains as many times 3 lines, as that space contains orbs of equal weight with that of the 36 fathoms, wherein M. *de la Hire* made his experiment. Now the number of these orbs may be so great, that the cylinder shall vastly surpass the magnitude of the terrestrial globe, as may be easily proved; for taking for instance the density of gold, which is known to be the heaviest of all bodies, and to be no less than 14630 times heavier than air in our orb, it will be easy to gather, that this air will be reduced to the same density as gold, by a column of mercury, containing 14630 times 28 inches, that is, 409640 inches, since in this case, the bulks of air will be in the reciprocal ratio of the weights they are pressed withal, according to M. *Mariotte's* experiments and our own; and these 409640 inches will express the height of the barometer in the orb, where the air was reduced to the same density as gold, and the number 2 lines $\frac{2 \cdot 1 \cdot 6 \cdot 2 \cdot 2}{4 \cdot 0 \cdot 9 \cdot 6 \cdot 4 \cdot 0}$, the thickness to which the 36 fathoms of our orb would be reduced, that is, such thickness of an orb in that part, would be as heavy as ours; so that 'tis evident all the other lower orbs of air, of the same thickness, would weigh considerably more than if they were mercury; now to learn the number of these orbs, we need only cast an eye over

the following table, which contains reductions of the thickness of several orbs of air, equal in weight with our own, by different heights of mercury, which in these orbs would be those of the barometer, with their number and the depth they are to be below ours.

The orb on the surface of the earth, pressed by 28 inches of mercury, being 36 fathoms thick.

The 32d orb, 992 fathoms below the first, being pressed by 36 inches of mercury, will only be 28 fathoms thick.

The 68th orb, 1899 fathoms below the first, being pressed by 45 inches of mercury, would only be 22 fathoms, 2 feet, 4 inches, 9 lines, $\frac{2}{3}$ thick.

The 136th orb, 3213 fathoms deep, pressed by 62 inches of mercury, will only be 16 fathoms, 1 foot, 6 inches, 7 lines thick.

The 272d orb, 5026 fathoms below the first, being pressed by 96 inches of mercury, would only be 10 fathom 3 feet thick.

The 544th orb, 7312 fathoms below the first, being pressed by 164 inches of mercury, would only be 6 fathom, 10 inches, 6 lines, $\frac{7}{64}$.

The 1092d orb, 9850 fathoms below the first, being pressed by 301 inches of mercury, would be 3 fathoms, 2 feet, 1 inch, 1 line $\frac{1}{3} \frac{1}{6} \frac{2}{1}$.

The 2184th orb, 12580 fathoms below the first, being pressed by 564 inches of mercury, would only be 1 fathom, 4 feet, 6 inches, 5 lines $\frac{1}{3} \frac{1}{6} \frac{4}{4}$.

The 4368th orb, 15492 fathoms below the first, being pressed by 1120 inches of mercury, would only be 5 feet, 4 inches, 9 lines $\frac{6}{11} \frac{7}{2} \frac{2}{0}$ thick.

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The 8736th orb, 18404 fathoms below the first, being pressed by 2212 inches of mercury, would only be 2 feet, 8 inches, 9 lines $\frac{1}{2} \frac{1}{2} \frac{2}{3}$ thick.

The 17472d orb, 21194 fathoms below the first, which is that where cork would be sustained, being pressed by 4396 inches of mercury, would only be 13 inches, 9 lines $\frac{1}{4} \frac{1}{2} \frac{2}{3}$ thick.

The 78960th orb, 28595 fathoms below the first, which is that where oil would be sustain'd, being press'd by 19768 inches of mercury, would only be 3 inches, 8 lines, $\frac{1}{9} \frac{1}{7} \frac{2}{5}$ thick.

The 82208th orb, 28744 fathoms below the first, which is that where wax would float, being pressed by 20580 inches of mercury, would only be 3 inches, 6 lines $\frac{6}{20} \frac{1}{8} \frac{2}{80}$ thick.

The 84112th orb, 28836 fathoms below the first, which is that where wine would be sustain'd, being press'd by 21056 inches of mercury, would only be 3 inches, 5 lines $\frac{2}{10} \frac{6}{10} \frac{1}{6}$ thick.

The 86128th orb, 28929 fathoms below the first, which is that where water would be sustain'd, being pressed by 21560 inches of mercury, would only be 3 inches, 4 lines, $\frac{8}{21} \frac{1}{60} \frac{2}{60}$ thick.

The 124880th orb, 30408 fathoms below the first, which is that where honey would be sustain'd, being pressed by 31248 inches of mercury, would only be 2 inches, 3 lines $\frac{2}{3} \frac{7}{12} \frac{1}{4} \frac{6}{8}$ thick.

The 638064th orb, 39910 fathoms below the first, which is that where tin would be sustain'd, being pressed by 159544 inches of mercury, would only be 5 lines $\frac{2}{1} \frac{1}{9} \frac{2}{44}$ thick.

The 689808th orb, 40208 fathoms below the first, which is, that where iron would be sustain'd, being pressed by 172480 inches of mercury, would only be 5 lines $\frac{2}{1} \frac{1}{24} \frac{2}{80}$ thick.

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The 776048th orb, 40708 fathoms below the first, which is that where copper would be suspended, being pressed by 194040 inches of mercury, would only be 4 lines $\frac{2}{19} \frac{2}{40} \frac{1}{40}$ thick.

The 890960th orb, 41202 fathoms below the first, where silver would be sustained, being pressed by 222768 inches of mercury, would only be 3 lines $\frac{2}{22} \frac{2}{27} \frac{6}{68}$ thick.

The 991648th orb, 41551 fathoms below the first, where lead may be sustain'd, being pressed by 247940 inches of mercury, would only be 3 lines $\frac{1}{24} \frac{2}{79} \frac{2}{40}$ thick.

The 1172528th orb, 42181 fathoms below the first, which is that where mercury would be sustained, being pressed by 293160 inches of mercury, would only be 2 lines $\frac{2}{29} \frac{1}{31} \frac{2}{60}$ thick.

Lastly the 1638448th orb, 43528 fathoms below the first, which is that where gold would be sustained, being pressed by 409640 inches of mercury, would only be 2 lines $\frac{1}{40} \frac{1}{96} \frac{1}{40}$ thick.

It must be observed, that tho' in this table, the orb, where mercury should be sustained, is said to be 3 lines thick and upwards; yet, in reality, it must not be so much. The reason is, that the experiment upon which the calculus of this table is founded, were made by several persons; some of whom found the weight of air to that of mercury, as 1 to 10800; and others, as 1 to 10470, in lieu of which ratios, it may be proper to take that of 1 to 10368, which is the mean between the other 2, and corresponds exactly to that of 3 lines to 36 fathoms; besides, that this last is much more commodious for calculation. On this footing, the orb, where mercury would be sustained, would only be the 1161104th its depth below ours 41931 fathoms, and the number of
inches

inches of mercury, it would be pressed by 290276.

As to the ratios of air to other bodies expressed in this table, they were found from the ratio of 1 to 770, which is that found by *M. de la Hire* between the weights of air and water, and a table of weights given by *M. Blondel* in his mechanicks; that of cork was found by experiment, the heights of mercury expressed in the table being to each other, as the numbers which express them divided by 28, we need not here give those ratios, whereof these same numbers are the equimultiples.

You may now clearly perceive, that at 43528 fathoms depth, the air would be at least $\frac{1}{4}$ heavier than mercury, we say at least, by reason the depths assigned to these orbs, are rather too great than too little. *Dr. Halley*, in the place already quoted, shews, that these depths are represented by spaces, comprehended between a hyperbolical line, its asymptote, and 2 perpendiculars to the asymptote, representing the productions of the same bulk of air by heights of mercury, whereof the part of the asymptote, comprehended between such perpendiculars, is the difference; whereas for the easier calculating, we have here supposed the hyperbolical side of this space to be a right line, which gives those depths somewhat greater than they should be, though the difference is not very considerable.

Now this depth of 43528 fathoms is only the 4th part of the semi-diameter of the earth, which would contain many millions of millions more orbs, equal in weight with ours; supposing that the density of the air as not limited to that of the heaviest bodies known among us beyond this depth, that vast sphere of 6451538 fathoms in diameter still remaining of the terrestrial globe, may

may only be full of air prodigiously condensed, and vastly heavier than the heaviest bodies on the surface of the earth.

But we have shewn by the experiments related in the memoirs of 1702, that the more the air is compressed, the more will the same degree of heat increase the force of its spring, and render it capable of the greater effect; that the heat, for instance, of boiling water will increase the force of this spring beyond what it is in the degree of heat, which, in our climate, is called temperate, by a quantity equal to $\frac{1}{3}$ of the weight it is pressed withal, which is enough to induce us to believe, that a degree of heat, which, in our orb, is only capable of a very moderate effect, becomes capable of a very violent one in the lower orbs; and as there are degrees of heat in nature, vastly greater than that of boiling water, 'tis possible, there may be some, whose violence being thus assisted by the weight of the air, may be more than sufficient to break and overturn this solid orb 43528 fathoms thick, wherein all the bodies we are acquainted of are contained, and whose weight, however enormous it may seem, is only a trifle in comparison of the rest.

But if it be obvious, what effect heat would produce in these lower orbs, we do not so easily conceive, how it could be communicated thither otherwise than by descending through the upper orbs, for want of other issues; since the air of these orbs being extremely condensed, can only contain very few particles of fire in its intervals; and that as this condensation grows excessive towards the centre of the earth, it must be almost wholly void of air in that part. This sentiment, 'tis true, stands quite opposite to that of M. Des-
cartes, and to the system of the central fire;
but

but this alone should not make it be discarded, till by other experiments, more convincing than those we here go upon, it be shewn erroneous.

V. *Remarks on the table of the degrees of heat in the philosophical transactions, for April 1701, read by M. Geoffroy in the assembly on Tuesday, July 24, 1703. by M. Amontons * ; translated by Mr. Chambers.*

As to the first article, we do not see why the author should fix the first degrees of heat and cold to the moment when water begins to freeze, since there are other liquors which freeze more or less readily than water, and which might have had preference thereto; nor do we see, what could oblige him to make the distinction of hot and cold degrees, since every quantity of heat may be called either hot or cold, according as it is compared, either to extreme cold or intense heat. The degree of cold, for instance, where water congeals, being a considerable degree of heat, when compared to extreme cold; be this as it will, the degree of heat, which the author lays down as the first in his scale, is the same, as that marked on my thermometer for 51 inches, 6 lines; and that which he calls 34 degrees and $\frac{1}{2}$, which he says, boiling water cannot pass, is that marked on my thermometer at 73 inches; so that if from these 73 inches, we subtract 51 inches and $\frac{1}{2}$, the difference will be 21 inches $\frac{1}{2}$, which being divided by 34 $\frac{1}{2}$, will give 7 lines $\frac{1}{2}$ of my thermometer for each degree of this author. On this footing, what he calls the degree of heat of air in winter, would extend from 51 inches and $\frac{1}{2}$, to 52 inches, 8 lines $\frac{2}{3}$.

* Aug. 4, 1703.

W^hat

What he calls the degree of heat of the air in spring and autumn, would extend from 52 inches, 8 lines $\frac{2}{3}$, to 53 inches, 11 lines $\frac{2}{3}$, and his temperate would be at 53 inches, 4 lines $\frac{2}{3}$, which is 7 lines $\frac{1}{3}$ below ours, that is, below the temperature of the vaults of the observatory.

What he calls the degree of heat of summer's air, extends from 53 inches, 11 lines $\frac{2}{3}$, to 55 inches, 10 lines $\frac{2}{3}$, which is 2 inches, 9 lines $\frac{1}{3}$ below our greatest heat, and the whole compass which he allows the variations of air, between the heats of summer and winter, is only 3 inches, 8 lines $\frac{2}{3}$; whereas with us, it is found 5 inches, computing, as he has done, from the degree wherein water freezes, which is far from being the utmost degree of cold felt in our climate, any more than 56 inches, 6 lines, are the greatest heat. The whole extent of this difference being usually in the open air, and without sun about 8 inches, which induces us to think, that the author made his observations in a close place, and comparing his temperate with ours, it readily appears, that his observations were made in the colder climate. 'Twere to be wished, the author had told us something hereof, and had expressed the subterraneous temperature of the place of his observations.

The degree, which he calls the degree of heat of noon-tide air in the month of *July*, and which probably determined with him the heat of the air in the summer's solstice, is much below that found here, his only reaching to 55 inches, 10 lines $\frac{2}{3}$, and ours to upwards of 58 inches.

The degree, which he calls the greatest degree of heat, that the thermometer is capable of receiving from the natural heat of the human body, corresponds to 58 inches, 11 lines $\frac{2}{3}$, of my thermometer; and the experiments I have made on this

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head, would convince me he had determined it very accurately, if it were not certain, that this natural heat is always the same both in summer and winter.

When the thermometer stood at 29 inches, 5 lines, several persons, whose pulses were as under-mentioned, made the thermometer rise by the heat of their hands, to the heights expressed in the other column.

Pulses in a minute. *Heights of the thermom.*

70	58	inch. 6 lin.
74	58	7
70	58	5
66	58	9
56	58	7
68	58	2
60	58	5
80	58	6

One of these persons putting the thermometer in his mouth, could not make it rise higher than by the heat of his hand.——By the way, it may be observed, that the pulsation of the arteries bears no relation to the natural heat, and that the one cannot be estimated from the other.

As to the 7th and 8th article, whose degrees of heat, as they are marked, correspond, *viz.* the 7th to 60 inches, 8 lines of my thermometer, and the 8th to 62 inches, 1 line; it doth not appear, that any thing certain can be determined thereon, by reason that all men are not equally sensible, and I have had much ado to keep my hand any time in the bath of the 7th article, when my servants supported that of the eighth. For a considerable while the degree of heat, where the bath neither appeared to me hot nor cold, was

was 58 inches, 5 lines, which is precisely that to which the heat of my hand would make the thermometer rise; the thermometer then standing at 56 inches.

The degree of heat of a bath, wherein melted wax poured therein begins to fix and loose its transparence, was found the same as the author has represented it, and corresponds to 64 inches, 1 line, with my thermometer. — The degree of heat of a bath, wherein he says a piece of wax will melt, answers to 66 inches, 5 lines $\frac{1}{2}$ of my thermometer; but in my experiment, a piece of white wax, 10 or 12 grains heavy, would not melt intirely in water lower than 67 inches, 3 lines. — A piece of tallow, of the same weight, melted at 61 inches, 10 lines, and a piece of butter at 59 inches, 9 lines.

As to the 2d column of his table, which contains the same degrees of heat in a geometrical expression, it seems to be founded upon a false principle, which is, that water beginning to freeze has no degree of heat; whereas we find several other bodies, whose heat suffices to maintain their fluidity all that time; and thus so far is the heat of boiling water from being almost triple of the natural heat, or the degree of fusion of wax double thereof; that 'tis more probable those degrees of heat are only to each other, as the numbers $59\frac{1}{2}$, $66\frac{2}{3}$, and 73, which express the force of the spring of the air, occasioned by those degrees of heat, when it has not liberty to extend, but is pressed in the boiling water with 73 inches of mercury.

What the author says of the hot iron, where-with he found the degrees of heat, which could not be had by the thermometer, is not very intelligible: his words are, *Calor quem ferrum ca-*

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lesatum, corporibus frigidis sibi contiguis, dato tempore communicat; hoc est, calor, quem ferrum dato tempore mittit, est ut calor totus ferri; which is certainly to be understood of the heat remaining; and all we can conceive to be meant by it is, that the quantity of heat it communicates in a certain time, is equal to what is left; so that a hot iron, which, in a certain time, had lost $\frac{1}{2}$ its heat, would only loose $\frac{1}{2}$ of that $\frac{1}{2}$, that is a $\frac{1}{4}$ in a second time, equal to the first; an 8th in a third time; a 16th in a 4th time, and so of the rest: but this reasoning seems to suppose, without any grounds, that the ratio of two to one reigns continually in this decreasing progression, whereas any other ratio, as of 3 to 1, or 4 to 1, may equally obtain, according as the air which surrounds the iron, and to which it communicates its heat, is more or less cold; or as the iron is more or less hot, and the times of cooling longer or shorter; all which circumstances may be diversified to infinity, and will occasion a like diversity in the ratios of the progression, whose terms are to express the different degrees of heat. So that to make use of this method to advantage, we should have as many tables of logarithms, as there may be different geometrical progressions; add that we are not afraid that the air, which continually succeeds around the hot iron all the time of its cooling, is of the same temperature, not to mention the necessity of sustaining this hot iron by some body, to which it will communicate more or less of its heat, as such body is more or less cold, or its mass greater or smaller.—— Being unable, therefore, to determine any thing precisely in this manner, I made use of another.

I put a bar of iron 59 inches long, and weighing 30 lb. almost upright upon charcoal in a furnace,

nace, where there might be the value of a bushel, which done, I lighted the charcoal, and took care to add the quantity of another bushel, and two several times in proportion, as the other wasted; and when the iron was heated, so that the lower end was quite white hot, it ceased to be red at the distance of 5 or 6 inches, and at the distance of 42 inches, was just sufficient to put butter in fusion. Upon this I laid it down horizontally, with the hot end on the fire, and the other supported by a piece of wood, and laying the following matters on it as carefully as I could, I found that thin glass would melt at 4 inches $\frac{1}{2}$ from the lower end.

Lead at 8 inches $\frac{1}{2}$; gunpowder kindled in the same place.

Tin melted at 11 inches.

Solder made of three parts of lead, two of tin, at 12 inches.

Drops of water boiled at 22 inches.

Virgin wax melted, at 30 inches 8 lines.

Tallow at 39 inches.

And butter, as already mentioned, at 42 inches.

Now if we consider that the spaces on this bar, comprehended between the boiling water, the fusion of wax, tallow, and that of butter, are to each other as the spaces marked on my thermometer, between the same degrees of heat; it will easily appear, how all the other degrees of heat, found by means of the bar, may be reduced into degrees of my thermometer. This I have effected in the following table, where on the one side are found all the degrees of heat, which I have learned by my own experiments; and on the other, those delivered in the philosophical transactions, both of them reduced into degrees of my thermometer, that they may be the easier compar'd together.

All that here remains, is to examine what the author has said of the rarefactions of air, linseed oil, and spirit of wine, with regard to which he seems to have been grossly overseen, at least there is a wide difference between us on this head, as well as in the experiments with the red-hot iron. The rarefaction of air, says he, under an equal degree of heat, was ten times greater than the rarefaction of the oil; and the rarefaction of oil, almost fifteen times greater than that of spirit of wine.

On this footing the rarefaction of air should be 150 times greater than that of spirit of wine, under the same degree of heat, which I have found true by experiment; for in my air thermometer, its bulk at the time when water freezes, is to its bulk in boiling water, as $148 \frac{2}{3}$, to $149 \frac{2}{3}$, in the author's thermometer, made with linseed oil. These bulks are to each other, as 10000 to 10705, or as 14 to 15; and in my spirit of wine thermometer, these same bulks are to each other as 472 to 515, or as 11 to 12; now these augmentations of bulk; $1 \frac{1}{4}$, $1 \frac{1}{4}$, $1 \frac{1}{4}$, are to each other as the numbers 77, 814, and 1036, where we find, that instead of the air's rarefaction, under an equal degree of heat, being ten times greater than that of linseed oil, it appears by this experiment, 10 times and $\frac{1}{2}$ smaller, and 14 times smaller than that of spirit of wine, which is far from being 150 times greater than that of spirit of wine; 'tis true, the author does not tell us in what manner he observed this great rarefaction of air, and that in my experiment, with the air thermometer, the air is not only continually pressed with the weight of the atmosphere, but in the time of freezing, compressed likewise with a column of mercury, 23 inches and

$\frac{1}{2}$ high, which in all amounts to 51 inches and $\frac{1}{2}$; and that this column is continually increasing, so that when the water boils, the column is 73 inches high; but supposing the air were only pressed by the weight of the atmosphere, its bulk, according to M. *Mariotte's* experiment, could only be increas'd in the ratio of 103 to 146; in which case, our rarefactions would be to each other as the number 4757, 814, 1036; where it may be observed, that the rarefaction of air, under an equal heat, can at most only be 5 times greater than that of spirit of wine. As to the rarefaction of linseed oil, so far is it from being 15 times greater than that of spirit of wine, that it evidently appears, by the above experiments, to be less than that of spirit of wine, or even of brandy, for our spirit was no more in the ratio of 814 to 1036.

A scale of several degrees of heat, found both by the resistance of the thermometer, and of red-hot iron, compared with those in the philosophical transactions, for *April* 1701, both the one and the other, express'd by the number of inches and lines of mercury high, which such degrees of heat would enable the air inclosed in a glass thermometer to sustain; supposing this height sustain'd in boiling water, pressed by the atmosphere, to be 73 inches.

Degrees

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Degrees of heat,
taken from the
philos. transact.

Degrees of heat,
found by expe-
riment.

Degree of heat of			Degree of heat of	
<i>inch.</i>	<i>lin.</i>		<i>inch.</i>	<i>lin.</i>
51—	6	winter's air, when wa-	51—	6
51—	6	ter begins to freeze.	51—	6
52—	2	} Degrees of heat of	52—	4
52—	9		53—	2
52—	9	} Degrees of heat of	53—	2
53—	4		54—	0
54—	0		54—	10
54—	0	} Degrees of heat of	54—	10
54—	7		55—	8
55—	2		56—	6
		Degree of heat of		
		the air, in the middle		
55—	2	of July.	56—	6
		The greatest degree		
		of heat which the ther-		
		момeter can receive		
		from the heat of the		
59—	0	human body.	58—	9
		That of a bird brood-		
59—	0	ing on its eggs.		
		The greatest heat of		
		a bath, which the hand		
		is able to endure, by		
		keeping it continually		
60—	8	stirring.		
		The greatest heat of		
		a bath, which the hand		
		can sustain for any con-		
		tinuance, without stir-		
62—	1	ing it about.		
		Heat of a bath,		
		wherein melted wax		
		cast in it begins to fix		
64—	1	and lose its transparence	64—	1
			Degree	

Degree of heat of the bath, wherein my hand neither felt heat nor cold.

58— 5

Degree of heat of a bath, wherein a piece of wax, remains in fusion without ebullition.

66— 5

67— 3

Degree of heat of a bath, wherein a piece of tallow melts.

61— 10

Degree of heat of a bath, wherein a piece of butter melts.

59— 9

Degree of heat which makes water boil, and beyond which it cannot go.

73—

73—

Degree of heat, wherein a mixture of 2 parts of lead, 3 of tin, and 5 of bismuth fused, begin to harden again.

73— 0

Degree of heat, wherein red-hot iron cooling, ceases to make drops of hot water, or thrown thereon, boil.

73— 4

or

74— 0

Degree of heat, wherein the same red-hot iron ceases to make drops of cold water boil.

74— 6

The smallest degree of heat, wherein a mix-

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ture of 1 part of lead,
4 of tin, and 5 of bismuth, melt and remain fluid.

76— 7

The smallest degree of heat, wherein a mixture of equal parts of tin and bismuth melt.

81— 5

to

80—10

This mixture fixes as it cools.

The degree of heat capable of melting a mixture of 2 parts of tin, and 1 of bismuth; as well as a mixture of 3 parts of tin, and 2 of lead; and wherein the mixtures of 5 parts of tin and 2 of bismuth, and of equal parts of lead and bismuth, fix.

87— 0

The smallest degree of heat, wherein a mixture of 1 part of bismuth, and 8 parts of tin, is capable of fusing.

93—10

96— 4

to

95— 2

The degree of heat, which puts tin in fusion.

80— 3

The degree of heat which melts bismuth; as also the mixture of 4 parts of lead and 1 of tin; and wherein the mixture of 5 parts of lead and 1 of tin, fixes again.

102— 0

The

111— 4 The smallest degree
of heat, which melts lead. 82—

The degree of heat,
which puts glass in fu-
sion. 84— 7

The same is the de-
gree of heat of red-hot
iron.

The heat of a large
charcoal fire, which
turned iron extremely
red, and made it scale. 87— 7

Gun-powder took
fire with the same de-
gree of heat as melted
lead. 82—

Solder made of 3
parts of lead and 2 of
tin melted, at 79— 1

122— 6 The degree of heat,
wherein burnt bodies
cease shining in the
dark, and wherein heat-
ed bodies begin to yield
a little light, but so
weak, as hardly to be
perceivable.

The same degree of
heat will melt a mix-
ture of equal parts of
tin and regulus martis.

At the same degree
of heat, a mixture of
7 parts of bismuth, and
4 parts of regulus mar-
tis melted together, be-
gan to fix.

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The degree of heat, wherein burnt bodies afford light in the dark, but none in the crepusculum.

136— 3

At this degree of heat, the mixture of 2 parts of regulus martis, and 1 part of bismuth melted together, began to fix.

The same befel a mixture of 5 parts of regulus martis and 1 of tin.

The degree of heat, wherein melted regulus martis fixes.

142— 6

The degree of heat, wherein burnt bodies afford light in the crepusculum immediately after the sun's rising or setting, and none, or very little in the open day.

151—10

The degree of heat of a little sea-coal fire without blowing, as also the heat of iron turned as red as it could be in this fire.

171— 2

176— 1

to

181— 4

Degrees of heat of a moderate wood fire.

VI. *Barometrical experiments made upon several mountains of France, by M. Maraldi*.*

In a journey that we took with Mess. *Cassini*, *Chazelles*, and *Couplet*, under the direction of M. *Cassini*, for the determination of the meridian, we made some barometrical experiments on several mountains of *Auvergne*, *Languedoc*, and *Roussillon*, the heights of which above the surface of the sea, we measured geometrically. These new experiments, which were made at much greater heights than any that were taken before, may serve to shew the properties and extent of the air, and how much it is rarefied at different heights from the surface of the earth.

We shall not here relate the particulars of the operations and calculations, which we were obliged to make, in order to find the height of these mountains. It will be sufficient to say, that having determined their distances, with regard to the points contained in the triangles of the meridian, we have at the same time observed their apparent heights, with regard to each other, and that we have made these operations by a series of uninterrupted observations from the mountains of *Berri* to the sea-coast of *Roussillon*. Then having observed the apparent height of some of the same mountains, and knowing their distance with the semi-diameter of the earth, we determined the perpendicular height above the level of the sea; and we concluded the perpendicular height of those which we did not see from the sea-coast, by the difference of the heights which

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had been observed between these mountains, and the rest, which had been already known.

To follow some order in the report of these experiments, we shall begin with the comparison of the observations made on the barometer at the same time, at *Colioure*, a town of *Roussillon*, and at the observatory, in the western tower of the great hall. This comparison serves to know the different height at which the barometer keeps at the same time in these two different places, to determine the height of the observatory above the surface of the *Mediterranean*; and consequently to know by the observations made at the observatory, what the height of the quicksilver was at the sea-coast during the time of our making the observations on the mountains, when we could not make them at the same time at the sea-coast.

During our stay at *Colioure*, from *Feb. 19*, to *March 12*, of the year 1701, to make the necessary observations for the meridian, we placed the barometer in a house, situated on a rock by the sea-side, in a place that was elevated 69 feet above the level of the sea. By the comparison of these observations made during one month, we find, that at *Colioure* the quicksilver was usually 3 lines $\frac{2}{3}$ higher than at the observatory. If we suppose the height of the air, which agrees to a line of quicksilver at the sea-side, to be 10 toises, as M. *Mariotte* supposes it, in his second essay on the nature of the air, by the difference of the height of the quicksilver, observed at the same time at the observatory and at *Colioure*, of 3 lines and $\frac{1}{3}$, we shall have the great hall of the observatory more elevated than *Colioure*, by 33 toises and $\frac{1}{3}$; which being added to 11 toises $\frac{1}{3}$ difference of height, between the level of the sea, and the place where the barometer was observed, make

make the great hall of the observatory 45 toises higher than the *Mediterranean*, and the height of the quicksilver less by 4 lines $\frac{1}{2}$ at the observatory, than at the sea-side. This height of the hall of the observatory differs but one toise from that which M. *Picard* has determined of the level of the ocean, which thereby appears to be the same level with that of the *Mediterranean*.

After these observations made at *Colioure*, we carried the barometer upon a summit of the mountains near *Rouffillon*, called *la Messane*, the height of which above the surface of the sea was measured geometrically 408 toises. On this mountain we found the height of the barometer 25 inches, 5 lines. We had found it some hours before at *Colioure* just 28 inches. The difference is 2 inches, 7 lines; to which, if we add 1 line $\frac{1}{6}$, for the height of the place where the barometer was, we shall have 32 lines diminution of the quicksilver for the height of 408 toises.

We made another experiment with the barometer on the top of *Bugarach*, a mountain of *Languedoc*, the height of which above the level of the sea, has been determined three different ways to be 648 toises. The barometer on the top of the mountain held suspended at 23 inches, 8 lines $\frac{1}{3}$, at the same time that it held at the observatory at 27 inches, 3 lines; to which, if we add 4 lines $\frac{1}{2}$, which are due to the height of the observatory, above the level of the sea, we shall have the height of the quicksilver reduced to the same level of 27 inches, 7 lines; the difference of which, from 23 inches, 8 lines $\frac{1}{3}$, is 46 lines $\frac{1}{3}$, which answer the height of 648 toises.

In *October*, we observed on the top of the mountain of *la Costa*, near the *Mont-d'Or*, in *Auvergne*, the height of the quicksilver to be

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23 inches, 4 lines. It was observed the same day at *Paris* to be 27 inches, 10 lines, the difference of which is 4 inches, 6 lines; to which, if we add 4 lines $\frac{1}{6}$, due to the height of the observatory above the level of the sea, we shall have 4 inches, 10 lines, for 850 toises, which the top of this mountain is elevated above the surface of the sea.

From *la Costa*, and several other points of the meridian, we see the *Puy Domme*, a famous mountain near *Clermont*, in *Auvergne*, by the barometrical experiment, which M. *Perier* made on its summit; which is also related in M. *Paschal's* *Traité de l'Equilibre des Liqueurs*. This observation, which is the first that was known to be made on this subject, and is considerable for the great variation of the quicksilver, from the foot of the mountain to the summit, was indeed sufficient to prove, as was proposed, that the pressure and weight of the air, are the cause of the suspension of the quicksilver; but it would serve but imperfectly to find the height of the atmosphere, as has since been attempted, because the height of the mountain above *Clermont*, was determined only by guess, and its height above the surface of the sea was entirely unknown. By the same method that we have used to find the height of other mountains, we have supplied the principal circumstance that was wanting in this observation, having determined its height above the level of the sea, to be 810 toises, 40 toises lower than the mountain of *la Costa*. By the comparison which we have made of M. *Perier's* experiments, having had regard to the different height of the quicksilver between *Paris* and *Clermont*, which result from the observations made a year after that of *Puy Domme*, and to the

the falling of the quicksilver from *Clermont*, quite to the top of the mountain, we find that at the height of 810 toises, which is the height of *Domme*, as we measured it, there would be a diminution of 4 inches, 11 lines, with regard to the surface of the sea, which would be a line of diminution more than we found upon *la Costa*.

This difference may come from the reductions which we are obliged to make, as there were no observation made at *Paris* on the day of the observations, which should have been, because of the variation which often happens in the height of the quicksilver from one day to another; and indeed it may come from the difficulty of being always exact in such nice experiments.

The observations, which we have related, added to those which were made in 1672, by M. *Cassini* at *Notre Dame de la Garde*, near *Marseilles*, and to those which M. *de la Hire* made 10 years afterwards upon mount *Clairat*, near *Toulon*, have served us to find a rule, according to which the quicksilver diminishes at different heights of the air above the surface of the sea, which agrees very well with the observations, and is easily retained.

Suppose, that at the sea-coast the height of the air, agreeing with a line of quicksilver, is 61 feet, which make 10 toises, 1 foot, within $\frac{1}{60}$ part of M. *Mariotte's* hypothesis; that the height agreeing with the second line, is 1 foot more than the first; the third a foot more than the second, and so on.

According to this hypothesis, from the surface of the sea, to the height of 178 toises, the quicksilver should fall 15 lines $\frac{2}{3}$. M. *Cassini's* observation, which was made at this height, gives 16 lines $\frac{1}{3}$. At the height of 257 toises, the hypo-

thesis makes the diminution of the quicksilver 21 lines $\frac{8}{9}$. M. de la Hire's observation makes it 21 lines $\frac{1}{2}$. At the height of 408 toises, according to the calculation, the quicksilver should fall 32 lines $\frac{1}{6}$, which agrees with the observation of *la Massane*. At the height of 648 toises, the hypothesis makes the falling of the quicksilver 46 lines $\frac{2}{3}$, instead of 46 lines $\frac{1}{2}$, according to the observation of *Bugarach*. At the height of 851 toises, it is 57 lines $\frac{2}{3}$, instead of 57 $\frac{1}{6}$, which results from the observation of *la Costa*.

By this rule, which represents all the observations hitherto made within less than a line, we may know within a few toises, by the observation of the barometer, the height of a mountain, the elevation of the places of the earth above the surface of the sea, tho' at a great distance from it, and how far one place is elevated above another, to the height of near half a league above the surface of the sea, which is the height where our observations end.

Following the same principles, and supposing the height of the quicksilver at the sea-coast to be 28 inches, as it commonly is, we have calculated what the height of the atmosphere should be, in this manner. If from the height of the quicksilver, which, at the sea-side, is 28 inches, or 336 lines, you subtract the half, which is 168, and multiply it by 337, the number of lines of the suspension of the quicksilver augmented by unity, and divide the product by 6, the quotient will be 9436 toises; which being added to 3360 toises, due to 336 lines in the proportion of 10 toises for each, there will be 12796 toises, which make 6 leagues $\frac{1}{2}$ for the height of the atmosphere. If we calculate in like manner the height of the last line but one, we shall find that the air, which answers

swers to the greatest height, would there be above 6 times more rarefied, than the air is at the sea-side.

We do not pretend to give for the exact height of the atmosphere, that which we have just found by the calculation ; it is an attempt that we have been willing to make, to see what results from our observations made at much greater heights, and in much greater number than what we had before.

We know how difficult it is to make a just conclusion of the measure which we have made of the rest of the atmosphere, which is incomparably greater, even though the upper air should be of the same constitution with that which is near the earth. But besides these difficulties, there are also other considerable ones, there are others which may come from the variations which happen to the barometer in the same country, and from the different variation, which is found in different climates.

For we have observed, that the weight of the air varies considerably in the same places at different times ; that it is usually more heavy in fair serene weather, and lighter when it is cloudy and full of vapours ; which seems so opposite to the judgment that we naturally make of it, that before these experiments some celebrated philosophers made no difficulty to suppose the contrary.

A great number of experiments made some time ago in *Spain, Italy, and England*, and compared with those which we made at the same time at the observatory, have shewn, that the barometer varies in the same circumstances of time ; and what is very considerable, these variations happen most commonly on the same days, especially those which are quick and sudden. We have

found, that the variations of the barometer are greater in the northern than in the southern countries. We have observed, that in *Sweden* they are $\frac{1}{3}$ of the greatest height of the barometer; that they are greater there than in *France*, where they are but one $\frac{1}{7}$; that in *France* they are also greater than between the tropicks, and toward the equinoctial, where they do not arrive to $\frac{1}{5}$ part. We have also observed, that the barometer, situated at a little height above the surface of the sea, always remained lower in the observations made near the equinoctial, than in *Europe*; so that if we suppose the height of the air above the surface of the quicksilver to be proportioned to its suspension in the barometer, the height of the atmosphere would be greater towards the N. pole, than near the equator.

To obtain more certain informations concerning the extent of the air by experiments made at great heights, we know none more proper than such as should be made on the *Canigou*, which is the most southern mountain of the *Pyrenæans*, where the triangles of the meridian are terminated. It is higher than the mountains of *Auvergne*, *Languedoc*, and the *Pyrenæans*, which we have observed. It is also nearer to the sea-coast, from whence it is seen, not being above 10 leagues from it; so that the experiment might be made at the same time on the sea-coast and on the mountain, without having occasion for any reduction. The height of the *Canigou*, above the surface of the sea, measured 2 different ways, has been found to be 1440 toises, which are a little less than $\frac{1}{4}$ of a league in perpendicular height, which, according to the hypothesis, would give 7 inches of diminution, which make $\frac{1}{4}$ of the greatest elevation of the barometer.

rometer. We had determined this height, as well as that of several other mountains, in the design of making barometrical experiments upon it; but we were not able to execute it, because of the great quantity of snow which covered them at the time of our observations.

As the principal difficulty in these experiments consists in knowing the height of the places where they are made, we have thought it proper to give here a catalogue of the principal mountains, of which we have found the height above the surface of the sea, to give an opportunity to the learned near these mountains, to make barometrical experiments upon them, and to see if the hypothesis proposed answers to their observations. It would be of use also, to make upon some of these mountains, some observations for the refraction of the stars. The experiments which M. *Cassini* had made at *Marseilles*, and agree with those which we made last, having given the height of the air much greater than that which results from the hypothesis, which he had used in determining the astronomical refractions, gave him room to conjecture, that there might be some fluid matter dispersed in the lower part of the air, and a little elevated above the highest mountains of the earth, which was the principal cause of the refractions of the stars. These observations made both at the sea-side, and on the highest mountains, would serve to know whether this refractive matter, different from that of the air, is really in nature; whereas at present he proposes it only as a commodious invention for the calculation of the refractions.

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A catalogue of the principal mountains, of which we have measured the height above the surface of the sea.

	Toises.
The Massane in Roussillon	408
Bugarach in Languedoc	648

Mountains of Auvergne.

The Puy Domme	810
The Costa	851
The Puy de Violent	853
The Cantal	984
The Mont d'Or, which is the highest mountain of Auvergne	1030
The Mont Ventoux, in the Comtat of Avignon	1036

Mountains of the Pyrenæans.

The mountain of S. Bartholomew, in the country of Foix	1185
The mountain of Mouffet	1258
The Canigou	1440

VII. *Observations on a petrified brain, by M. du Verney, jun. **

Almost three months ago an ox broke loose four times, just as the butcher was going to knock him down. This remark is not useless, because it shews, that it was no weak and faint animal; but on the contrary, very strong and vigorous. When they came at last to open his head, the brain resisted the cleaver. The butcher thinking he had struck against the iron ring, to which the head was fastened, redoubled his

* Nov. 14, 1703.

blow,

blow, but to no purpose; and having in vain struck a third and fourth time, he was obliged to take a hammer, and break the skull in pieces to get out the brain. After he had mashed the skull with his hammer, he and his companions were surpris'd to find a great stone instead of a brain. They brought it to me; and I was still more surpris'd than they, when I saw this prodigy. As all the parts of the skull had been broken in pieces, it was impossible for me to replace every part.

The figure of this brain is singular, and very different from that of a common brain. Its surface is all over uneven and rugged; not much unlike a piece of rock-work. The two great lobes are thicker and closer than they are naturally. Each of them has a process at the upper part; that of the left lobe, which proceeds about a finger's breadth beyond the surface of the rest, is rounded; that of the right lobe, which rises about an inch, is larger, and forms a sort of saddle, terminated at each extremity by a pommel, of which the hinder one is round like a little button, and the other is much bigger and broader, and has a sort of groove in the middle. These two eminences leave a considerable space between them, which is continued down below the base of the skull, forming from space to space unequal cavities, of a greater or less bigness, of which the two more considerable ones may contain a small filberd. The lower part of the left lobe makes a blunt process, which advances beyond that of the right-side. The different windings, which form the furrows of the brain, seem in some places larger than usual, and also separated, leaving sinuosities and depressions, which probably were hollowed by the motion

tion of the vessels, which were there preserved; and also on the interior *lamina* of the skull, there are furrows formed by the course of the vessels. In several places of the lobes of this brain, there are windings also, which are still joined by the *pia mater*, which is hardened and petrified almost every where else, as well as the brain; only within the hinder part of the lobes, which joins to the lateral part of the *cerebellum*, there is also a tender and spongy substance, which was not hardened and petrified like all the rest, which is easily felt by introducing a probe; for this part does not resist the probe like the rest; and that place is properly what is called the third cavity, or the third ventricle, above which the pineal gland is situated, which is at the meeting of the *sinus's* of the *dura mater*. Tho' the eminence in this place is bigger than a walnut, its situation gives room to believe, that it is the pineal gland. This gland is in a manner articulated by its anterior and superior part, by the two processes of the lobes which we have described; and by its posterior and inferior part with the *cerebellum*. At the lower part of this gland there are several cavities, the most considerable of which, at the superior and posterior part of the right lobe, forms a great sinuosity, which passing over the right-side of the *cerebellum*, terminates in a great cavity, about a finger's breadth in diameter, where the beginning of the *medulla oblongata* must be. Above the same place there is also a slight depression, about the size of a *denier*, which depression is also covered with its membrane, and appears transparent.

At the base of the skull, and by the side of this cavity, near to another pretty considerable sinuosity, there is also a tender, spongy substance, like

like marrow ; for in introducing a probe, it not only penetrates very easily to the depth of 5 or 6 lines, but in drawing it back, we find it all covered with this marrow ; and what is very surprising, is, that the neighbouring parts are all as hard as marble.

The *cerebellum* is situated obliquely, rising forwards upon the left lobe ; and the *processus vermiformis*, which process is naturally bent back and hid under the lobes, is here found elevated above the left lobe. Between the same lobe and the same process there is a considerable cavity, of an irregular figure, approaching to an oval, which penetrates very forward into the substance of the brain, and at the place where it ends, seems less hard, and only ossified. The *cerebellum* has almost preserved its natural figure, and there is no considerable cavity found in it, but only small depressions and *sinus's*, which seem to have been formed by the separation of the furrows.

In the base of the brain, which was cut by the butcher, we may see the grey and the white part distinctly, tho' they are quite petrified.

After the circumstances, which I have just related, I believe no one can question, that this brain really belonged to a living animal. If it had been all stone, it might have been said to be a production like those bones, which some take for fossils, and to these wonderful petrifications, which are sometimes found in the earth, and are, if I may so speak, sports of nature. But in this petrified brain some parts are bony ; and there is still a tender and spongy substance at its base ; there is also a substance in it like marrow, with which the probe, that has been introduced, is covered. All these facts prove invincibly,

cibly, that this brain is no production of chance or art, as those fossil bones and some artificial productions may be. The singularities in this brain, are characters of truth, which chance cannot imitate, and art knows not how to imitate.

Tho' examples are not necessary to authorise a thing so evident; I have been delighted to find in the works of a celebrated author, a fact like this, which confirms it; I mean the famous *Bartholin*, who relates it in his *anatomical centuries* *. He says; that in his time, in *Sweden*, as a butcher was going to kill an ox, and had split his skull, the brain resisted the blow, and made the cleaver fly back; that the mass of this brain was found to be petrified; that the ox always hung his head down when alive; that he grew very lean, which determined his master to sell him to the butcher. *Bartholin* adds, that he had not seen this brain; but that *Steno Bielke*, the *Swedish* ambassador, from whom he had received the information, had assured him that he had seen it in Count *Oxenstiern's* house, where it was kept, and that this ambassador had promised him to send a very exact figure, and a piece of it. The difference between our observation and that of *Bartholin*, is, that the ox, which he speaks of, was extremely lean, whereas ours was fat, and in good condition.

But how shall we answer the objections, which these observations furnish against what has always been thought on so important a subject? *Bartholin* confesses himself very much puzzled by it, *The brain*, says he, *must no longer be reckoned among the noble parts, since its functions are not absolutely necessary to life.* Thus is the brain degraded from the nobility, which it has

* Lib. VI. Hist. 91.

hitherto enjoyed. I agree with this author, that the affair is very puzzling; but yet upon due consideration, I believe we can give some solutions to these difficulties.

If all the parts of our ox's brain were equally petrified, and all over as hard as they are in a great number of places, it would be very difficult, not to say impossible, to explain how the animal could have lived; the communication of the brain with all the other parts of the brain being intercepted, and the spirits, which it must continually send for the nourishment and enlivening of the parts, not being any longer able to pass. But the soft and spongy substance, which is also found in some places, as has been observed, gives light into the establishment of some probable conjectures. We may say, that this marrow-like substance, has always been able to furnish a certain quantity of spirits, not only to cause the nerves of the brain to perform their ordinary functions; but also to serve for the nutrition of all the other parts. And what gives room to believe it, is, that in the base of the skull, there were also found some nerves, which seemed to be in their natural state, as the spinal marrow also was. We cannot question but this was sufficient for all these different functions, if we consider, that some persons have been seen, who have had the substance of their brain very much injured, even to the base of the skull, and yet have performed all their actions as freely as before, for a certain time, because the nerves had not suffered any disorder. Thus tho' the greatest part of our ox's brain was petrified, yet the animal lived, because the nerves could receive and distribute the spirits, or perhaps prepare them themselves. We shall be easily led to embrace this

opinion, if we recollect what several authors have written, that children have been born at their full time without any brain; and M. Mery also, whose merit is well known, has shewn me the skeleton of a child, which had neither brain nor spinal-marrow, and yet had all its nerves distributed as usual.

The remark, which *Bartholin* has made in speaking of the observation already mentioned, confirms what I say, that the tender and spongy substance, which is found in some parts of the brain of our ox, has been able to furnish some spirits to the nerves. For this author finding it difficult to conceive how the ox, which had its brain petrified, could live till he was knocked down by the butcher, and suspecting that there must be in the brain some open *sinus's*, thro' which the animal spirits passed freely from the arteries and nerves; he was confirmed in this opinion by M. *Bielke*, the *Swedish* ambassador, who assured him, that in several places of the brain there were holes, which would easily admit a straw. After all, in what manner soever the ox was able to live, it must be acknowledged that this petrified brain is a sort of prodigy; as we see every day, that slight wounds made in the brain, or only in its membranes, cause general perturbations, which deprive it of all its functions.

An explanation of the figures.

Plate II. Fig. 1. AAAAA. The circumference of the brain.

BBBB. The two great lobes.

CC. The processes, which rise above the upper part of the brain.

DDD.

DDD. The space, which begins between these two eminences, and is continued below the base of the skull.

E. A blunt process, which proceeds beyond that on the right side about a finger's breadth.

FFFF. Several windings separated and half opened, which form sinuosities and depressions.

GGGG. The eminence, which I take to be the pineal gland, both by its figure and situation; and by its insertion, by means of which it has preserved a sort of play between the *cerebrum* and *cerebellum*.

Fig. 2. represents the brain seen sidewise.

HHHH. The *cerebellum*.

I. The anterior *processus vermiformis*, which is raised above the left lobe.

L. The posterior *processus vermiformis*.

M. The cavity between the *cerebellum* and the process of the upper part of the left lobe; the bottom and sides of which seem less hard, and only ossified.

N. The inferior process of the left lobe.

O. The superior process.

PP. The pineal gland, situated in such a manner, that its articulation with the *cerebrum* and *cerebellum* is easily seen.

RR. The superior process of the right lobe, made in form of a saddle.

S. A portion of the left lobe, which was cut by the butcher.

Fig. 3. represents the same brain seen in its upper part, where we see different cavities, which go to the base of the skull, marked TTT.

VIII. *Remarks on the inequalities of the motion of pendulum clocks, by M. de la Hire*.*

The astronomers, who have taken great care to regulate the motion of their pendulums at seconds by the motion of the stars, have observed some inequalities in them, which they have not been able to reduce to any certain rules. I made some remarks on these inequalities, in the memoir, which was read by me to the academy, and printed in the memoirs of 1700†, and amongst the rest, on those which may come from a little spring plate, which I had put instead of silk, to sustain the pendulum; for I had thought, that this plate not being subject to the alterations, which happen to the silk by the dryness and moisture of the air, the vibrations of the pendulum might be much more equal; but, at last, I was obliged to take away the plate, and put the silk again, because I observed much greater inequalities than before; and I afterwards found, that the clock went pretty true, sometimes not varying from the mean motion a single second in 4 days, wherein the pendulum makes 345,600 vibrations. But I have also sometimes observed, that from one day to another there were alterations, considerable enough to embarrass an exact observer, and to exercise a philosopher, who would search out the cause, which must necessarily be physical.

The different states of the air seem to be the sole causes of the alterations, which we observe in the motion of the pendulums: for it is hot or cold, dry or moist, light or heavy, thick or thin; all these different qualities mixing toge-

* Dec. 15, 1703. † Vol. I. Pag. 221 of this abridgment.
ther

ther in different degrees, may cause great alterations in the motion of clocks. But to find any thing that is to happen, we must consider these different states separately.

We suppose, in the first place, that if the cycloid is well made, according to the rules which M. *Huygens* has given, all that can accelerate or retard the motion of the wheels, will not make the clock alter, since there could only happen longer or shorter vibrations, which would not fail to be isochronical, or of equal duration. Thus cold being able in some measure to fix the little oil that is fastened to the pivots of the wheels, will cause their motion to be more difficult, than in a warm season, when the oil shall be more liquid, and consequently the vibrations will become shorter; but yet they will be of equal duration with those which are longer, being rectified by the figure of the cycloid.

The moisture that clings to the wheels and pinions, may cause almost the same effect, without any inequality happening to the motion.

But tho' the cycloid is the necessary figure to make the longer or shorter vibrations be isochronical, it should be considered, that it could have place only when the suspension has no bigness or thickness, which is impossible in the execution; wherefore, as we make use of a twisted silk, thick enough to sustain the lentil of the pendillon or pendulum, which is heavy, and as we must neglect nothing that may contribute to the justness of this motion, the figure must not be a cycloid, but a line parallel to the cycloid, which may be distant from it toward the concave side, by half the thickness of the silk, that the *axis*, or middle of this silk, may describe the cycloid exactly, as I have explained in my *treatise of*
epi-

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epicycloids, which may serve for the motion of
machines.

We may also observe, that the little threads of
filk, which compose the twine, are dry and stiff,
and that they may consequently suffer altogether
considerable alterations, almost like those of the
plate spring, which is stiffer in cold and dry wea-
ther, and softer in hot; but it is an accident that
we cannot avoid, when we make a flexible sus-
pension for the pendulum; wherefore we might
try what I proposed in the above-mentioned
memoir.

If we consider the different states of the air,
with regard to the pendulum, and not the wheel-
work of the clock, we shall observe so many
different accidents, that we should hardly be able
to think a clock can go equally for an hour toge-
ther, during which the pendulum makes 3600
vibrations.

We know, that the heat of the sun in summer
is strong enough to heat a bar of iron of 6 feet
long, and to make it longer than it was in win-
ter, when exposed to the frost, by $\frac{2}{3}$ of a line,
as I found by a very exact experiment, which I
made formerly. Wherefore these two different
states of the air, with regard to the length of the
pendulum, which should be 3 feet, 8 lines $\frac{1}{2}$, to
swing seconds, might alter $\frac{1}{3}$ of a line, which
would cause a very considerable difference in the
duration of the vibrations of the pendulum, since
it might go as far as 32" in a day. But as this
case could not happen, but when the clock is ex-
posed to the air and sun in these two seasons,
which is not frequent, we do not often observe
such great alterations. There sometimes happen
pretty considerable differences of heat from one day
to another, and from night to day, to make the

rod of the pendulum lengthen or shorten, which may retard or accelerate the motion of the clock some seconds, as we also sometimes observe, which may come from this cause alone. Wherefore in the use that is made of pendulum clocks for celestial observations, where it is necessary to know the hour with the utmost exactness, they must be placed where they are as much sheltered as possible from all the injuries of the air.

The moisture, dryness, density and rareness of the air, may also cause considerable alterations in the motion of the pendulum. For when the air is moist, that is, when it is filled with a quantity of small particles of water, which remain suspended in it, or when it is dense or thick, the pendulum will find a difficulty in cleaving it, and its vibrations must then be of much longer duration, than when it is dry or rare. For we know by experience, that a very light feather falls in a tube, out of which the air has been pumped, almost as soon as a stone does in the air. But as we must not judge of what is to happen on these occasions, without making the experiment if possible, I have thought that if the moist and thick air can make the durations longer, than a dry and thin air, we should perceive a very great difference between the motion of the pendulum in air and in water. To know what it was, I made a pendulum of half seconds, with a ball of lead weighing 2 ounces, which was suspended by a fine thread, and I put it in motion in the water. I observed, at first, that the great vibrations were quickly shortened, and that the motion sensibly diminished after a little more than a minute. But, as I was persuaded, that these vibrations in the water must be at least each of them a second, which were but $\frac{1}{2}$ a second in the air,

I was very much surpris'd to find, that they appeared to me almost as quick, or of equal duration, with those which were made in the air. To measure them exactly, I caus'd the vibrations of the clock pendulum of seconds to be counted, during one minute, and at the same time I counted the vibrations of the pendulum of half seconds, in the water of a great broad vessel, where the ball was immerg'd about half an inch, and I found, after having several times repeated the same experiment, that the pendulum in the water made but 112 vibrations instead of 120, which it should have made in the air, in one minute.

I made also the same experiment with a single pendulum of seconds, the ball of which was of lead, and weigh'd 5 ounces, and I found, as in the other, that the great vibrations continued but a very little while, and that the pendulum almost stopp'd entirely, after 2 minutes. But it made no more than 114 vibrations in the water, whilst the pendulum of the clock made 120 in the air in two minutes. Thus the retarding caus'd by the water in the vibrations of the pendulum, is 3" in a minute. I could have been glad to make the observations of these differences of vibrations in the water and air for 20' or 30', to know more exactly their difference, and to see what proportion there was in the retarding of the vibrations in the water, in these pendulums of different lengths, but I could not proceed any farther.

As a pendulum of seconds loses in the water 3" in a minute, it would lose in a day 4320". But if we suppose, that this diminution of the motion of the pendulums, comes from the density of the *medium*; and if the air is dense by the weight with which it is loaded, without having regard to the quantity of particles of water mix'd in it, it will

will follow, that if the weight of the air changes only $\frac{1}{8}$, as is pretty often observed in the barometer, $\frac{1}{8}$ of 4320" of retarding the pendulum in the water for one day, which is 154", will be the diminution or retarding of the clock in the space of a day, with regard to these two different states of the air. But we have never observed so great a difference as that in pendulum clocks; we cannot say therefore, that the different weights, with which the air may be loaded, can cause its different densities; or else we must confess, that its different densities have not the same effect on the motion of the pendulum, as the density of the water has, which may proceed from the different configuration of the parts of these two bodies, of which those of the air, tho' very close and compact, may be easily separated; and, on the contrary, those of the water not without great difficulty, being adherent to each other. We may add also, that the last vibrations in the water, being shorter than the first, go faster.

It would be for this reason also, that the air, tho' filled with particles of water, would but little, if at all, retard the motion of the pendulum, because all these particles not being connected, but quite separated by the particles of the air, may be very easily displaced between the particles of the air, wherein they float.

But if these particles of water do not retard the motion of the pendulum, by the difficulty of being displaced, they may cause a considerable alteration by another mean. If the air, from being dry, becomes moist, it is certain that a very great quantity of these particles of water must stick to the surface of the rod, and of the weight of the pendulum, and they may even penetrate them a little; and they will consequently make

a sort of covering upon the rod and lentil of the weight, which will have its centre of oscillation different from that of the compound of the rod and weight; wherefore the centre of the oscillation being then different from what it was before, the duration of the vibrations will not be the same as it was. Not that we can in any degree remedy this accident, in using for a pendulum a cylinder with a very small base, which is homogeneous in its whole length, and being suspended by the extremity of its *axis*, would very near have the same point for the centre of oscillation of its surface and of its body, and consequently whatever alteration happened to this surface, provided it was equal in all its parts, the motion of the pendulum would not be sensibly altered. It would be the same thing, if instead of a cylinder we made use of a parallelepiped, and provided it be also suspended by the extremity of its *axis*.

Lastly, if the cycloid was ill made, it might cause new irregularities in the motion of the pendulum, according as its vibrations should be longer or shorter, of which several others would be formed, by their combination with the first.

As for the different lengths of the pendulum in different climates, it seems to me, that some remarks might be made upon it; for M. *Picard* had observed, at *Uranibourg* and at *Bayonne*, where I was with him, that the length of the simple pendulum of seconds, was exactly the same as at *Paris*. Great attention was given to this observation of *Bayonne*, because it was known what M. *Richer* had related at *Cayenne*. *Uranibourg* and *Bayonne* are distant from each other in latitude more than 12° ; and between *Bayonne* and *Cayenne*, the difference of latitude is

38° ,

38°, for *Cayenne* is almost at 5° of N. lat. which gives only a difference almost triple of the first, for which we find $\frac{1}{4}$ of a line of diminution of the length of the pendulum. We must therefore conclude, that this difference of length does not become very sensible, till we approach the line.

But several years afterwards M. *Varin*, *des Hayes*, and *de Glos*, having been sent towards the line, to make some astronomical observations there, found that in the island *Gorée*, which is in 14° N. lat. the length of the simple pendulum at seconds, ought to be 2 lines shorter than in *France*. The observations made at *Cayenne* and *Gorée*, leave no room to doubt of their being made with great certainty and exactness in all the circumstances. However, if we would have concluded this difference of the length of the pendulum for *Gorée* by that of *Cayenne*, we should have said that of *Gorée* ought to be only about $\frac{1}{4}$ of a line shorter than at *Paris*, whereas by the observation it is 2 entire lines. On the contrary, if we had concluded that of *Cayenne* from that of *Gorée*, we should have said about 3 lines, and it is found to be but $\frac{1}{4}$ of a line.

These great differences cannot by any means agree with the *hypotheses*, which M. *Mariotte* has formed in his *Traité du mouvement des eaux*, and M. *Huygens*, in his *treatise of light*, and we must seek for others to explain, why the length of the pendulum is the same in the latitudes of 55° $\frac{1}{4}$, and 43° $\frac{1}{2}$; why it is 2 lines shorter in 14° $\frac{2}{3}$; and why at 5° it is only $\frac{1}{4}$ of a line shorter. But might we not suspect, that this different length of the pendulum is not real, but only apparent, and that it comes only from the mea-

measure that we have made use of. For it is very true, that metals, and all bodies in general, are extended considerably by heat, and contracted by cold. *M. Picard* says, that in the length of a foot, he has observed a prolongation of $\frac{1}{4}$ of a line; and consequently it should be $\frac{1}{4}$ of a line in the length of the pendulum, whereas I have found it but $\frac{1}{3}$ of a line. This difference might come from the different ways, in which the observations were made; for *M. Picard* having exposed bodies to the frost, put them afterwards near the fire, and for my part, I only exposed them to the sun the summer following. We might therefore say, that towards the line and between the tropics, where the heats are very great, metals extend and lengthen very considerably, beyond what they do in these countries; and perhaps also by a particular cause of the vapours and exhalations, which penetrate them, as we know they are very penetrating in those countries; and lastly, more at one time than another, and more in one place than another. Wherefore these causes of extension, which are not considerable in these countries, may be very different at *Gorée* and *Cayenne*, and at different times; for we are persuaded, that towards the tropics, the heats are much stronger than towards the line. And if the iron rod of 3 feet, measured at *Paris*, at the time of *M. Richer's* departure, was prolonged $\frac{1}{4}$ of a line at *Cayenne*, he must have found the length of the simple pendulum at seconds, measured with this rod, shorter than at *Paris* by $\frac{1}{4}$ of a line, tho' in reality it was the same in both places. In like manner, if at *Gorée* the measure was prolonged 2 lines more than it was at *Paris*, the length of the single pendulum at seconds, would have appeared shorter

shorter than at *Paris*, by 2 lines. This seemed to me the most probable about this *phenomenon*. If it was so, the universal measure of the pendulum would remain always the same, over all the earth, and the particular measures should be regulated by this measure, taking the length of the simple pendulum for 3 feet, or $\frac{1}{2}$ a toise.

An examination of the demonstration, which Mess. Mariotte and Huygens give of the different lengths of the simple pendulum of seconds, in different parts of the earth.

We have here, according to M. *Mariotte*, only to demonstrate, whether bodies fall more slowly under the equinoctial than any where else; and whether they fall faster as they approach nearer to the poles. This is what he proposes to do in his *Traité de mouvement des eaux*, p. 245. supposing the motion of the earth about its *axis*.

He says, the motion of the earth gives the air an impresson, which makes it endeavour to fly from its *axis*, with a velocity proportionable to that of its motion; and that this motion being greater towards the equinoctial than towards the poles, the effort which it makes towards the equinoctial, is greater than that which it makes towards the poles; and it is from this different effort, that he concludes that the bodies, which are in the air, are repelled and driven from the earth with more force near the equinoctial, to hinder them from falling, than when they are near the poles.

This reasoning of M. *Mariotte*, is founded only upon the supposition that the air, which surrounds the earth, is repelled from it by its motion about its *axis*; having perhaps been persuaded

suaded of this effect by a common experiment, that if we move an irregular body in the air, the air being struck by its inequalities, endeavours to fly from the body in lines perpendicular to the motion of the body; but it seems to me, that the same thing cannot happen to the globe of the earth, supposing its daily motion about its own *axis*.

For in the first place there is too little earth, and the inequalities of it are too small, in proportion to the smooth surfaces of the waters, to part the air sensibly from the earth, and consequently the sole motion of the surface of the earth would make all the bodies of this surface shock the air with as great a velocity, as that of these bodies, which we might take for a very violent wind from E. to W. which however would have no determination to fly from the surface of the earth, and the particular causes of the winds could not have force enough to resist it. If we perceived between the tropics any motion from E. to W. there are very often great calms also; and we might give other physical reasons for this motion, besides that of the earth; and moreover, what proportion is there between the velocity of this wind, and that of the surface of the earth, which travels 9000 leagues in a day.

It must therefore be allowed, that the atmosphere, which encompasses the earth on all sides, makes as it were but one body with it; and in the supposition of the motion of the earth about its *axis*, the atmosphere is carried along as its surface. Whence it follows, that a stone, which should fall in this atmosphere, would not receive any impression from the motion of the earth, as it would happen to a ball of lead let
fall

fall in a vessel full of water, whilst the vessel should be carried along by a very quick horizontal motion; for there is no doubt but this ball would fall to the bottom of the vessel in the same place where it would fall, if the vessel was at rest, since the water contained in the vessel is at rest there, with respect to the mass of water, and the sides of the vessel whilst it is in motion.

And if it was possible for the air to be thrown off from the surface of the earth, by the motion of the earth, as M. *Mariotte* supposes, whether by a tangent, which would fly off from E. to W. or by a *radius* of the centre towards the circumference, the weight of the pendulum, which descends and rises again in the same vibration, which goes on one side in one vibration, and on the other in the other, will always be as much accelerated in ascending, as retarded in descending, and as much accelerated on one side, as retarded on the other; whence it follows, that no alteration can happen from this cause in the duration of the vibrations of the pendulum.

But in short, if we should allow M. *Mariotte* all that he pretends to conclude from his *hypothesis*, it would always follow, that for the nearest degrees to the poles, the augmentation of velocity of the motion of the pendulum would be much greater than for the degrees towards the equator; since this augmentation would be in the proportion of the diminution of the motion of the matter, which would be that of the sines of the complement of the degrees of latitude, which diminish much faster as we approach the poles, than towards the equator, which is contrary to the observation made at *Uranibourg*, and at *Bayonne*, and also contrary to the irregularity, which is found between *Cayenne* and *Gorée*.

As for M. *Huygens*, who did not print his *treatise of light*, till several years after M. *Ma-*

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riotte's Mouvement des eaux was published, he
says, *we cannot question its being a sign that bo-*
dies descend more slowly near the equinoctial than
in France. This is also what M. Mariotte had
supposed, and for his demonstration he adds, that
he found as soon as ever this new phænomenon
was communicated to him, that the cause of it might
be referred to the motion of the earth, which being
greater in each country, according as it approached
nearer to the equinoctial line, must produce a
greater effect in throwing off bodies from the centre,
and thereby taking from them a certain part of
their gravity: It is easy to see, by his own
words, which I have just related, that he makes
use of the same hypothesis with M. Mariotte,
and he afterwards determines the quantity of the
diminution of this effort by his third theorem *de*
vi centrifuga. Wherefore all the reasons which I
have related against the explanation of this phæ-
nomenon by M. Mariotte, would also serve as well
as this, which concludes only the same thing from
the same principle. Whence, I say, there must
be some other cause of this effect, which does not
depend on the motion of the earth.

As for what regards the observation, it seems
very easy to be done, since we may count the vi-
brations of the simple pendulum for one hour,
where it always continues in motion after it has
been once put in motion, and that if the pendu-
lum was to be two lines shorter, that which should
be two lines longer would make in an hour about
8 vibrations less than the other, which is a very
great difference to be mistaken in. It will be the
same thing in the other lengths in proportion. It
must however be observed, that if a thread of
the *pitta*, or silk grass, be made use of to sustain
the weight, let this thread be ever so slender, it
is

is always flat, and it happens, that the last vibrations usually become turning, from being strait as they were at first, as I have tried, because this thread cleaving the air obliquely in its motion, turns the pendulum one way in going, and another way in returning, which gives it by degrees a determination to turn. I have also observed, that these last turning vibrations, which ought to be shorter than the first, because they are of less extent, are of longer duration than the strait ones, which may impose in the observation. And if M. *Picard* has had regard to these turning vibrations, and these other gentlemen, who have observed near the line, have given no attention to it, they will have found much fewer vibrations in an hour in the simple pendulum, which turned a little, than there ought really to be, if the vibrations had not turned; and this will have made them judge, that the pendulum was to be shortened in order to make its vibrations in less time.

I shall now give an example of what I said before, that there are not such great alterations observed in the motion of the pendulum of the clock, as ought to happen from the different condensations of the air.

Observations on the barometer, and on the constitution of the air, from *Nov.* 22, to *Nov.* 29, 1703, in the morning.

Inches. Lines.

<i>Nov.</i> 22	barometer	27	$4\frac{1}{6}$	foggy weather.
23	—	27	1	rain.
24	—	27	$3\frac{1}{2}$	rain.
25	—	27	$9\frac{1}{6}$	fair.
26	—	28	$1\frac{1}{6}$	a thick fog
27	—	27	$11\frac{1}{2}$	a thick fog.
28	—	27	$9\frac{1}{6}$	a fog.
29	—	27	$7\frac{1}{6}$	fair.

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I have found, by the observations on the sun's passage thro' the meridian, and by the equation of the clock, that between the 22d and 25th, when the barometer was very low and at a mean height, and the weather partly rainy and partly fair, that the clock had gained upon the mean motion of the sun $9'' \frac{2}{3}$ in a day; and that between the 25th and 26th, when the barometer was very high, which shewed a very great condensation of the air, with a very thick fog, that the clock had gained but $6''$, which would shew a retardation of the motion of the pendulum of $3'' \frac{2}{3}$ in a day. But from the 26th to the 29th, when the barometer was always pretty high, with a great deal of fog, the clock had gained $9''$ in a day. Whence it is easy to see, that for a very great difference in the constitution of the air, when its weight joined with the fog ought to have greatly retarded the motion of the pendulum, as I have before observed, it was found however to be but a few seconds, which might be ascribed to other particular causes.

IX. *A way to raise a great vessel upon the hold, as it is constructed in the port of Toulon, without making use of any machines, by M. de la Hire *.*

There must be on each side of the hold a trench, in which the water is every where about six feet deep, and large enough to contain small vessels or barks according to their breadth; and these vessels, if ever so much loaded, must draw no more water than the six feet in the trench.

* Dec. 22. 1703.

The great vessel, which we would raise upon the hold, having been drawn against the hold, we must place on each side 2, 4, or 6 of the little vessels, as many as shall be necessary for the following operation, and fill them with as much water as they will bear without sinking.

Then place great masts across the breadth of the great vessel, passing beyond both sides, to be sustained upon tressels placed, and rested upon the deck of the small vessels †. The masts must be well fastened to the body of the great vessel, either with chains or cables, embracing it underneath, and fastened to the masts or otherwise.

Which being done, begin to empty all the water out of the small vessels, which will proportionably rise towards the surface of the water; and raising the masts with them, will raise the body of the great vessel as high as they rise themselves.

In this state, the great vessel may be brought forwards with the small vessels, without any difficulty, till it comes to the hold, upon which it will be raised according to the elevation of the small vessels, and rested upon the hold in that place, in such a manner, that it cannot fall or slip towards the foot of the hold, when the small vessels sustain it no longer.

Which being done, fill the small vessels with water, as at first, and place other tressels than the first, being high enough to touch the masts, which cross the great vessel.

Now, if you empty the water out of the small vessels, they will rise, and also raise the masts as they did at first; and consequently they will also raise the body of the great vessel, to which the masts are fastened, in such a manner, that the

† Plate II. Fig. 4.

great vessel will no longer touch the hold at the place where it was, and it shall be drawn against the hold, as it was at first; but it will be much more elevated. It will be kept also in this place by means of the great cables fastened on the top of the hold.

By this means, repeating the operation as often as shall be necessary, we may make the vessel rise above the hold, and draw it entirely out of the water, provided the quantity of water, with which the small vessels are filled, is equal at least in bulk to that which the great vessel occupies at first, before it begins to rise upon the hold. And as we may easily measure the space or bulk, which the great vessel occupies in the water, we shall have by this means the magnitude and number of the small vessels, to be used in raising the great vessel.

When I said the trenches on both sides of the hold should be six feet deep in water, it was only to establish a mean measure; for if the water is deeper, we may use larger barks to raise the vessel; and if it was not possible to make them of this depth, they must be broader, and in greater number, that is, as many as the length of the vessel would permit, being ranged on one side, according to their length.

It must be observed, that, when the vessel has begun to rise upon the hold, the fore-part will be more elevated than the back-part; wherefore the small vessels, which shall be placed on the side towards the fore-part, must have higher treffels to support the masts in that place, than those which are towards the hinder part, that the whole body of the vessel may rise in the same inclination, in which it was placed on the hold.

It

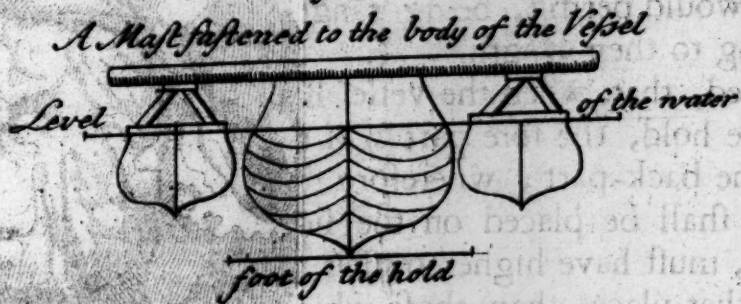
Fig. 1.



Fig. 3.



Fig. 4.



J. Mynde sc.

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It will be also very necessary to put a cradle under the vessel, to keep it upon the hold, without leaning to one side or the other, and to defend it from accidents, which may happen to it from its own weight.

X. *A description of the heart of a sea tortoise, by M. Mery* *.

The vital parts of this tortoise were contained together with the natural parts in the same cavity. The lungs occupied the upper part of it entirely. They were fastened to the back, from the neck quite to the tail; the heart was placed in the fore-part, and the natural parts behind. There was no diaphragm to make a partition between them.

The heart of this tortoise was however inclosed in a *pericardium*, to the bottom of which it was fastened by three little fleshy ligaments. This *pericardium* was full of a liquor as clear and transparent as the purest water, in which the heart of this animal was bathed. Its figure was conical, it was 2 inches long, and 6 lines broad or thereabouts. On the inside it was divided into 3 ventricles, one was placed to the right, the other to the left, and the third in the middle, under the right ventricle.

The left ventricle was separated from the right by a fleshy partition, which had towards the *basis* of the heart an oval aperture, pretty like that which is found in the partition that divides the auricles of the heart of the human *fœtus*. This partition was also pierced by a great number of little holes, by which, as well as by the

* *Extracted from the registers of the royal academy of sciences for March 12, 1704.*

oval aperture, these 2 ventricles communicate together.

There were upon this oval aperture two depressed valves; but as in this state they did not shut entirely, they could only in part hinder the blood from passing out of one of these ventricles into the other by this hole.

The right ventricle communicated also with the middle one by another aperture. This was 5 or 6 lines long to 3 or 4 broad, in the middle of its length. In this passage from one to the other, there was no valve; and as this second hole of communication was in a manner as long as the ventricle of the middle one was deep, we can consider this only as a continuation of the right ventricle, from which it was distinguished only by a little contraction. The fibres, of which these 3 ventricles were composed on the inside, not being closely locked against each other, formed a sort of fleshy sponge in their capacity. The left ventricle was equal to that of the middle one; but the right ventricle seemed alone as big as the other two taken together.

Three trunks of arteries proceeded from the *basis* of the heart of this tortoise. Two of these arteries had their outlets into the right ventricle, and the third into the middle ventricle. These 3 vessels had each of them no more than 2 sigmoidal valves at their apertures.

The 2 trunks of arteries, which parted from the right ventricle, had their diameters almost equal; they were both composed of 2 plans of fleshy fibres, very visible, and lying one upon another. The fibres of the outer plan were disposed according to the length of these 2 arteries, those of the inner plan seemed to be circular.

These 2 plans of fibres were not sensible in the third trunk of an artery, which derived its origin from the middle ventricle; but the capacity of this alone was almost as great as that of the two others taken together, and its membranes also were less thick.

Of the 2 trunks of arteries, which proceeded from the right ventricle placed one by the side of another, the right advancing forwards divided immediately into 2 great branches. The first going in a right line towards the neck divided into 2 others, and each of these into 2 branches, 2 of which were extended into the anterior fins; these made the axillaries; the 2 others placed between the first went to the head, and formed the carotids.

The second branch, bending on the right-side, passed under the right branch of the *trachea*, after which it slid between the lungs, to gain the hinder part of the body. Taking this course, it gave branches to the kidneys, the bladder, the parts of generation, and the posterior fins. By this distribution of the arteries, it was very easy for me to judge, that this first trunk was that of the *aorta*, tho' it parted from the right ventricle. Its capacity was a little bigger than that of the artery, which I am going to describe.

The left trunk formed on its own side the same bending which was made on the right by the posterior branch of the *aorta*, and followed the same course. This trunk did not send any branch into the anterior parts of the body of this tortoise. It divided only beyond the liver into 3 branches, of which the first supplied the place of the coeliac, the second of the mesenteric, and the third passing from left to right, reunited with the posterior branch of the *aorta*, as the arterial

canal of communication does in the human *fetus*. And it is for this reason, that I have called this second artery the canal of communication, to distinguish it from the trunk of the *aorta*.

The third trunk, which drew its origin from the middle ventricle, made the body of the pulmonary artery. This trunk divided into 2 considerable branches, which formed to right and left bendings like those of the posterior branch of the *aorta*, and of the canal of communication. Both of them passed under the branches of the *aspera arteria*, to go one to the right, and the other to the left lobe of the lungs. The circuit of this third trunk was almost equal to that of the *aorta*, and of the canal of communication taken together. These 3 arteries were joined together by their exterior membranes, from the heart, to their division into branches.

All these roots of the veins of each lobe of the lungs uniting together, formed at their coming out of the lungs a vein on each side, the capacity of which was half as big as that of the 2 pulmonary arteries, which deserves attention. These 2 veins went to the left auricle, at the outlet of which they joined together by their extremity, without forming after their union a canal, which had alone the capacity of these 2 veins taken together ; thus they did not form any trunk, each of them poured the blood which it brought, immediately into the capacity of this auricle.

The veins, which brought back to the heart the blood of all the other parts of the body, did the same ; so that there was no single trunk, to which alone we can truly give the name of *vena cava*. For tho' in appearance they all formed by their union a crooked canal, joined to the auricles by its convex part, to the liver by its concave part, the

the middle of which answered to the aperture of the right auricle; yet this canal, in this place, seemed a little narrower than in its lateral parts. These 2 parts therefore made 2 distinct trunks, seeing in the place of their union they did not form a duct, which had alone the capacity of both joined together. The blood of both of them flowed immediately into the right auricle. The axillary veins, which opened into these 2 trunks, were filled with fleshy fibres, which formed, by their interlacing, a sort of tuft, of a wonderful structure, of which there were some rudiments in the conflux of the 2 *venæ cavæ*.

The right auricle had at its outlet 2 valves, which formed together an oval aperture 7 or 8 lines long, and 3 or 4 broad in the middle. This aperture made the communication of the veins just mentioned with this auricle.

These 2 valves are so much more worthy of observation, as there was none at the inosculation of the left auricle with the pulmonary veins.

Of the 2 auricles of the heart the right was the biggest, its capacity seemed double of that of the left auricle, which is observable, as well as the difference between the capacity of the pulmonary veins and arteries, to determine pretty nearly the quantity of blood which passes thro' these vessels, and its different velocity.

These 2 auricles were filled with fleshy fibres, which being connected together different ways, formed a sort of little net, and also little cells, which were pretty deep. These auricles were joined together on the outside; but on the inside they were separated by a partition, which was but $\frac{1}{2}$ a line in thickness. This partition was partly fleshy, and partly membranous.

Its membranous part, made in form of a half moon, falling perpendicularly upon the *basis* of the heart, united with it, and divided it into two; so that it divided the outlet of the right ventricle from that of the left.

At the membranous part of this partition, two semilunar valves were fastened and suspended. These valves being depressed, one into the right ventricle, and the other into the left, partly closed the *foramen ovale*, which made the communication of these two ventricles. In this state, these valves formed one cavity between them: being elevated they described a plane parallel to the *basis* of the heart, but in this situation they could stop no more than half the openings of the auricles into the ventricles, because of their being made in form of a half moon.

Of these two valves, that which possessed the entrance of the right ventricle had no companion, but that which was placed at the outlet of the left ventricle, was accompanied by two others much smaller than itself; these had not the liberty of rising, which the other had, because they were fastened to fleshy columns, which bound them within to the side of the left ventricle.

XI. *A description of the heart of a great American land tortoise, with reflections upon that of M. du Verney **, by the same.

The figure A †, which represents the heart of the *American* land-tortoise, reversed in the fore part, with the auricles and veins in their natural situation, resembles a kidney a little flattened at the top and bottom; so that it is much more broad than

* See pag. 137 of this abridgment.

† Plate III. Fig. 1.
long

long. Its basis † is a little concave, and is naturally turned towards the head of this animal.

The vital and the natural parts are inclosed in the same cavity, because there is no diaphragm to part them. M. du Verney seems to me therefore to be mistaken, when he says, that as the tortoise walks on its 4 feet, *the heart is situated in the upper part of the thorax above the liver*. It is naturally placed in the fore part of this cavity, which contains all these parts together; which does not hinder the heart from being alone, and particularly inclosed in a *pericardium*.

I have observed 7 veins near the heart ‖; the two *cavæ* BB, the two axillaries CC, the coronary D, and two other veins EE, which I call hepatic, because they derive their origin only from the liver. The four first are very considerable; the other three are much less so.

The two *venæ cavæ* proceed from the hinder parts of the tortoise, from which they bring back the blood to the heart. Passing by the liver, one to the right and the other to the left, they receive a great number of roots of veins from that bowel.

These two vessels do not form any particular trunk in the place of their concurrence. There, on the contrary, they seemed to have a little less capacity than before their union, tho' the coronary vein and left hepatic come hither. The right axillary and hepatic open into the right *vena cava* an inch from the right auricle: but the left axillary joins itself only on the side of the left *vena cava*, very near the entrance of this auricle. They are the two axillaries, that the jugulars are joined to, which bring back to the heart the blood of all the anterior parts; all these veins appear

† Fig. 2.

‖ Fig. 1.

to

to be merely membranous, their inner surface is as smooth as the outer. From the known structure of these veins, I draw two consequences against M. *du Verney's* description!

The first is, that he has not been able to see *about the heart of these animals, a sort of reservoir of an oblong figure, and pretty like that of a blown bladder, formed by the concurrence of several veins.* The second consequence is, that it is false, that this pretended reservoir is *lined on the inside with fleshy fibres, which cross one another, and interlace almost like those within the auricles of the human heart.* Nor is it true, that *the vena cava is lined in like manner thro' about an inch of its length, and the outlets of the other vessels.*

Fig. 1. which represents the seven veins just described, is a decisive proof, 1. That the great reservoir, which M. *du Verney* says is formed of the assemblage of these veins, does not exist in the land tortoise of *America.* 2. That the texture of fleshy fibres * existing only in the axillary veins of the sea tortoise, it is evident, that the observation made by this skilful anatomist, is certainly taken from the observations which I made on these veins in 1685.

The two pulmonary veins † FF do not form any more than the two *cavæ*, a particular trunk by joining together: on the contrary, the place of their union seems more contracted than any other part. It is therefore false, 1. That *they go to form a second reservoir, much smaller than the first, as M. du Verney shews in his figures.* And because the inner surface of these two veins is no less smooth than that of the *venæ cavæ*, it is also false, 2. That *the basin of his little reservoir is also furnished on the inside with fleshy fibres.* The

* Fig. 9.

† Fig. 8.

two reservoirs, which this skilful anatomist represents to us in 8 figures, and the texture of fleshy fibres, which he ascribes to them, are therefore at last quite imaginary.

The two auricles GG represent on their outside only one blind sack, lying transversely on the *basis* of the heart; but this sack is divided on the inside by a partition I †, into two cavities of different bigness. This partition is fleshy both in its upper and lower part.

The capacity of the left auricle * HHH is smaller by half than that of the right auricle KK †. In both of them there are a great number of fleshy fibres.

In the angle formed by the left auricle with the partition I, which separates it from the right, there is an aperture L *, by which the pulmonary veins discharge their blood into the capacity of the left auricle. This aperture being narrower within than without, is one of the causes that hinder the blood of these two veins from returning whence it came.

But because the outlet of the two *venæ cavæ* is not also placed in the angle formed by the same partition with the right auricle, which this angle might contract; nature has given this auricle two valves MM †, which leaving only a small slit between them produce the same effect, that is, they oppose the return of the blood from the right auricle into the *venæ cavæ*.

M. *du Verney* places these two valves at the outlet of his great reservoir; but since it does not exist, it is visible, that they belong to the right auricle, of which they occupy the entrance.

† Fig. 3 and 5.

* Fig. 3.

† Fig. 5,

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I have observed in the *American* land tortoise, which I dissected, 4 ventricles, which communicate with each other by 3 straits, which make the separation of them. To make a just description of both of them, I shall regulate myself according to the course of the blood, which traverses them.

The first ventricle P *, which I call the left ventricle, both on account of its situation, and because it receives the blood from the left auricle, communicates with the second q †, by the first strait R, which I call the *foramen ovale*; because it pretty much resembles that of the human *fœtus* placed in the partition, which separates the auricles of the heart one from the other, and because it has the same use.

The outlet of the left ventricle is furnished with 3 sigmoidal valves NNN. That of the right ventricle has but one O. These valves perform the office of the triglochine valves of the human heart. M. du Verney however tells us, there is but one valve at the entrance of the left ventricle; he is therefore mistaken in both.

The second ventricle Q, which I call the right ventricle, because it is situated on the right side, and receives the blood of the right auricle, communicates with the third S ‖, by the second strait T. Above this passage, in the right ventricle, there is a fleshy valve V, made in form of a half moon. This valve can shut but a small part of this strait; it has been hitherto unknown to M. du Verney. The valve O, depressed in the *foramen ovale*, which in this state permits the blood of the right auricle to enter into the right ventricle, has not escaped his exact-

* Fig. 3.

† Fig. 5.

‖ Fig. 6.

ness;

ness; but however, 3 out of these 5 valves have not come to the knowledge of this attentive anatomist.

The third ventricle S*, communicates with the fourth X**, by the third strait Y.

These 4 ventricles communicating together, and the left and right having no arteries to bring back the blood which they receive from the auricles, it is easy to see, that the blood of the pulmonary veins, must necessarily pass from the left ventricle into the right, and mixing with the blood of the *venæ cavæ*, must enter together into the third and fourth, to take the course of the arteries which part from these 2 ventricles.

According to M. du Verney, there are but 3 cavities in the heart of the land tortoise of *America*; but if we examine the heart of this animal itself, we shall find 4, without reckoning the auricles. This excellent anatomist is no less mistaken in the number of the ventricles, than in that of the valves, with regard to which, I shall make the following reflection, before I proceed to the arteries.

Of the 3 valves NNN*†, which are placed at the entrance of the left ventricle P, the middle one, and the valve O*‡, situated at the entrance of the right ventricle Q, can but partly close the *foramen ovale* R, by sinking on each side, and by rising, partly stop the passages of the auricles into these two ventricles in the land as well as in the sea tortoise.

M. du Verney pretends, however, that when these 2 valves are raised, they entirely close the inosculations of the auricles with the ventricles,

* Fig. 6.

** Fig. 7.

*† Fig. 3.

*‡ Fig. 5.

and that when they fall, they do not at all oppose the passage of the blood from the left ventricle, thro' the *foramen ovale* into the right ventricle, but neither of these observations agree with experience. These 2 valves are fastened to the membranous part of the partition of the auricles, which falling upon the *basis* of the heart, separates the outlet of the left ventricle from that of the right.

From the basis of the heart A *, proceed 3 trunks of arteries; the *aorta* 1, the canal of communication 2, and the pulmonary artery 3. The *aorta* and canal of communication derive their origin from the third ventricle S †. The pulmonary artery 3 is derived from the fourth ventricle X ‖. These 3 arteries have each of them but 2 sigmoidal valves at their outlets.

M. du Verney and I agree, that *three considerable arteries proceed from the basis of the heart*. But as to the origin of these 3 arteries, his observations are very different from mine, for he says, *two of these arteries open into the first cavity of the heart*, which by his own observations, makes the right ventricle, as it *receives the blood from the right auricle*. Now I find, that there goes no artery from this cavity, and that the 2 arteries, which according to him compose the *aorta*, one of which, according to me, makes the canal of communication, far from *opening into the first cavity of the heart*, as he pretends, proceed from the third ventricle; and that the *third artery, which is that of the lungs*, proceeds immediately from the fourth ventricle, and not from the *third cavity of the heart*, as he pretends. This mistake comes only from his

* Fig. 2.

† Fig. 6.

‖ Fig. 7.

not having perceived, in the land tortoise of *America*, the 4 ventricles which I have demonstrated to the academy in the heart of this animal.

The trunk of the *† aorta, at about the distance of an inch from the heart, divides itself into 2 considerable branches; one turns backwards, the other forwards. The posterior branch 4, bending from left to right, crosses the anterior branch 5, after which it continues its course towards the tail, and gives branches to all the posterior parts of the body.

The anterior branch 5 advancing towards the head divides into two branches, each of which subdivides into two others, which make the axillary arteries 8, 8, and the carotids 9, 9, which throw themselves into all the anterior parts.

The second trunk of an artery * 2, 2, 2, 2, which I call the canal of communication, because it discharges a part of the blood which it receives from the heart into the posterior branch 4 of the *aorta*, bending also backwards, but on the left side, at first crosses this branch, and after having produced the coeliac artery 6, and the mesenteric 7, it unites with it, and opens into its capacity.

The pulmonary artery † 3, which makes the third trunk, represented open, is divided into two considerable branches, which cross one another from their origin; so that the right passes into the left lobe, and the left branch into the right lobe of the lungs.

The trunks of these three arteries are of unequal capacity. That of the canal of communication 2, is a little smaller than that of the *aorta* † 1, but that of the pulmonary artery ** 3, is

*† Fig. 2 & 4.

* Fig. 4.

† Fig. 7.

‡ Fig. 6.

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itself almost as big as that of the *aorta*, and that of the canal of communication taken together, in both tortoises.

In the land tortoise, the pulmonary arteries have a capacity equal to that of the pulmonary veins. In the sea tortoise the pulmonary veins have a much less capacity than the pulmonary arteries.

An explanation of the figures in plate III.

Fig. 1. A. The heart reversed.

B.B. The *venæ cavæ*.

C.C. The axillaries.

D. The coronary.

E.E. The 2 hepaticks.

F.F. The 2 pulmonary veins.

G.G. The 2 auricles.

Fig. 2. A. The heart in its natural situation.

G.G. The auricles.

1. The *aorta*.

2. 2. The canal of communication.

3. 3. 3. The pulmonary artery, and its two branches.

6. The *coeliac*.

7. The mesenteric.

4. The posterior branch of the *aorta*.

5. The anterior branch.

8. 8. The axillaries.

9. 9. The carotids.

Fig. 3. A. A. The heart.

H.H. The left auricle open.

L. The aperture of the pulmonary veins.

N N N. The 3 valves of the left ventricle.

P. The left ventricle open.

R. The first strait, or *foramen ovale*.

6. The *coeliac*.

7. The mesenteric.

I. The partition of the auricles.

Fig. 4. 1. The trunk of the *aorta*.

2. 2. 2. 2. The canal of communication.

4. 4. The posterior branch of the *aorta*.

5. The anterior branch.

6. The coeliac.

7. The mesenteric.

8. 8. The axillaries.

9. 9. The carotids.

Fig. 5. A. A. The heart.

I. The partition of the auricles.

K. K. The right auricle opened.

M. M. The 2 valves of the inosculation of the veins with the right auricle.

O. The valve of the entrance of the right ventricle.

Q. The right ventricle opened.

R. The first strait, or *foramen ovale*.

T. The second strait.

V. The fleshy valve.

Fig. 6. A. A. The heart.

S. The third ventricle opened.

T. The second strait.

1. The *aorta* opened.

2. 2. The canal of communication opened.

4. The entrance of the posterior branch of the *aorta*.

5. The anterior branch.

6. The coeliac.

7. The mesenteric.

Y. The third strait.

Fig. 7. A. A. The heart.

G. G. The auricles.

X. The fourth ventricle opened.

Y. The third strait.

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3. 3. 3. The pulmonary artery with its two branches.

Fig. 8. D. Part of the coronary vein.

E. The outlet of the veins of the right auricle.

F. F. The two pulmonary veins.

G. G. The auricles.

Fig. 9. represents the texture of the fleshy fibres of the axillary veins of the sea tortoise.







TABLE

OF THE

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ROYAL ACADEMY of SCIENCES at
PARIS, for the Year MDCCIV.

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- I. **O***F the earthquakes in Italy.*
- II. **O***A discovery of a singular property in the thermometer.*
- III. *Of the cause of the heat of lions.*
- IV. *On the figure of the extrados of a circular vault, of which all the arch-stones are in equilibrio with one another.*

In the MEMOIRS.

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- III. *That all the barometers, double as well as simple, which have been hitherto constructed, act not only by the greater or less weight of the air, but also by its greater or less heat; and the means of preventing this defect for the future, in the construction of double barometers, and of correcting the error in the use of the simple barometers, by M. Amontons.*

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- VI. *Extract of a letter from M. Sarrafin, physician to the king, in Canada, concerning the anatomy of the castor, read to the academy by M. Pitton Tournesfort.*
- VII. *The description and use of a level of a new construction, by M. de la Hire.*

A N
ABRIDGMENT
OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1704.

I. *Of the earthquakes in Italy.*

M. MARALDI having communicated to the academy some accounts which he had received of earthquakes in *Italy*, we will here collect from them the greatest part of the natural philosophy which they contained.

The earthquakes in *Italy* began *Octob.* 1702, and continued till *July* 1703. The countries which suffered most by them, and where they began, are the city of *Norcia*, with those depending on it in the *ecclesiastical state*, and the province of *Abruzzo*. These countries are contiguous and situated at the foot of the *Apennine*, on the south-side.

The earthquakes have often been accompanied with terrible noises in the air, and these noises have been also frequently heard without any earthquake, even when the sky has been very serene. The earthquake on *Feb.* 2, 1703, which was the most violent of them all, was accompanied, at least at *Rome*, with a great serenity of the sky, and a great calm in the air. At *Rome*, it lasted half a minute; and at *Aquila*, the capital of *Abruzzo*, 3 hours. It overthrew the whole city of *Aquila*, buried under its ruins 5000 persons, and made great havock in the adjacent parts.

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The shakings of the earth have commonly been from N. to S. or very near it, which has been observed by the motion of the lamps in the churches.

It made two openings in a field, from whence, with violence, issued out great quantities of stones, which, having entirely covered it, made it barren. After the stones, there sprang up suddenly from their openings two spouts of water, whose height much exceeded the trees of this country, which lasted a quarter of an hour, and overflowed the neighbouring countries. This water is whitish, like soap-suds, and has no taste.

A mountain which is near *Sigillo*, a town 22 miles distant from *Aquila*, had a pretty large plain on its summit, surrounded with rocks, which were a kind of wall to it. Since the earthquake of *Feb. 2*, instead of this plain, there is a gulph made of an unequal breadth, the greatest diameter of which is 25 toises, and the least 20. The bottom of it could not be found, tho' they went 300 toises deep. At the time when this opening was made, flames were seen to proceed out of it, and afterwards a very great smook, which lasted 3 days with some interruptions.

At *Genoa*, the 1st and 2d of *July*, 1703, there were two small earthquakes. The last was felt only by persons who were at work on the mole. At which time the sea in the port fell 6 feet; so that the gallies in the *Darse* touched the bottom, and the sea continued thus low near a quarter of an hour.

The sulphurous water, which is in the road from *Rome* to *Tivoli*, diminished 2 feet $\frac{4}{5}$ in height, both in the bason and in the ditch. In many places of the plain, called *Il Testino*, there were springs and rivulets of water, which formed im-

impassable marshes, but all dried up. The water of a lake, called *Inferno*, diminished also 3 feet in height. Instead of the old springs, which ceased, there came new ones about a league distant from the first; so that in all likelihood those very waters have changed their course.

II. *A discovery of a singular property in the thermometer.*

It must appear something surprising, that when we cover the ball of the thermometer with our hand to warm the liquor, and make it ascend to the tube, this liquor begins to fall, and does not ascend above its first level till after this seeming irregular motion, which is so contrary to what might have been expected. M. *Amontons*, who spoke of this, on the occasion of his new thermometers, refers this motion, whereby the liquor falls at first, to the rarefaction which the heat of the hand causes even in the substance of the glass of the ball, before it can cause any in the liquor. The capacity of the ball therefore is augmented, and consequently the liquor in the tube sinks, till it has received warmth enough to rise, notwithstanding the increase of the capacity of the ball.

M. *Amontons* hath calculated upon exact experiments, how much the increase of this capacity may be. and he has found it to be but 1000th part. This 1000th part, by which the ball is augmented, and which is the quantity of the liquor which enters into it, or which sinks, will become so much the more visible on the tube, the narrower the capacity of the tube is when compared with that of the ball.

III. *Of the cause of the heat of lions.*

In the lion, the gall-bladder has several plaits or folds, and from thence M. *du Verney* has conjectured, that the bile being able to remain there a longer time, and exalt itself more, may perhaps be the cause of the great heat of this animal, and of the continual fever which is ascribed to it.

IV. *On the figure of the extrados of a circular vault, of which all the arch-stones are in equilibrio with one another.*

A vault, or semi-circular arch, being placed upon its two imposts, and all the arch-stones or *voussoirs* that compose this arch being cut and placed in such a manner, that their joints being prolonged meet all in the centre of the arch; it is evident, that all the arch-stones must be in form of wedges, wider at the top than at the bottom, by virtue of which they bear upon and sustain each other, and mutually resist the effort of their weight which determines them to fall. The arch-stone in the middle of the arch, which is perpendicular to the horizon, and is called the key of the vault, is sustained on each side by the two contiguous arch-stones, exactly as by two inclined planes, and consequently the effort that it makes to fall, is not equal to its weight, but is a certain part of it which is greater, as the inclined planes that sustain it are less inclined; so that if they were infinitely little inclined, that is, perpendicular to the horizon, as well as the key of the vault, it would endeavour to fall with its whole weight, would not be any more supported, and would actually fall, if the cement, which we do not here consider, did
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not hinder it. The second arch-stone, which is to the right or left of the key of the vault, is sustained by a third arch-stone, which by virtue of the figure of the vault, is necessarily more inclined to the second, than the second is to the first; and consequently the second arch-stone in the effort that it makes to fall, exercises a less part of its weight than the first. By the same reason, all the arch-stones, reckoning from the key of the vault, proceed always in exercising a less part of their total weight; and, in fine, the last, which is placed upon a surface horizontal to the impost, does not exercise any of its weight, or, which is the same thing, does not make any effort to fall, as it is entirely supported by the impost.

If we would have all the arch-stones make an equal effort to fall, or be *in equilibrio*, it is visible that each, from the key of the vault to the impost, exercising always a less part of its whole weight; as for example, the first exercising $\frac{1}{2}$, the second $\frac{1}{3}$, the third $\frac{1}{4}$, &c. there is no other way to make these different parts equal, than by proportionably increasing the wholes, of which they are parts; that is, the second arch-stone must be heavier than the first, the third than the second, and so on to the last, which must be infinitely heavy, because it makes no effort to fall, and a null part of its weight cannot be equal to the finite efforts of the other arch-stones, unless this weight is infinitely great. To apprehend this idea in a more sensible manner, and less metaphysically, we need only reflect, that all the arch-stones, except the last, cannot let any other arch-stone whatever fall without raising themselves; that they resist this elevation to a certain point determined by the greatness of their weight, and by

by the part of it that they exercise, that only the last arch-stone can let any other fall, without rising in some manner, and only by slipping horizontally; that the weights, so far as they are finite, bring no resistance to the horizontal motion, and that they do not begin to bring any that is finite, till we conceive them to be infinite.

M. de la Hire, in his *Treatise of Mechanicks*, printed in 1695, has demonstrated what is the proportion, according to which the weight of the *voussoir* of a semi-circular arch must be encreased, that they may all be *in equilibrio*, which is the surest disposition that can be given to a vault, to render it durable. Before this, the architects had not any exact rule, and were only guided by trying. If we reckon the degrees of a quadrant of the circle from the middle of the key of the vault to one impost, the extremity of each *voussoir* will belong to an arch as much greater, as it shall be farther distant from the key; and according to M. de la Hire's rule, we must encrease the weight of an arch-stone above that of the key, as much as the tangent of the arch of this *voussoir* exceeds the tangent of the arch of half the key. The tangent of the last *voussoir* necessarily becomes infinite, and consequently its weight also; but as infinity has no place in practice, it is reduced to charge the last arch-stones as much as possible, that they may resist the effort that the vault makes to separate them, which is called the *shoot*.

M. Parent has inquired what should be the outside bend or *extrados* of a vault, of which the *intrados* should be circular, and all the arch-stones *in equilibrio*, by their weight, according to M. de la Hire's rule; for it is plain, that all these arch-stones, unequal in a certain proportion, make

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on the outside a certain regular curve. He found it only by points, but in a very simple manner; so that by his method we might easily enough build a vault, of which we shall be sure that all the arch-stones will be *in equilibrio*.

A considerable effect of the inquiries of M. Parent is, that he has at the same time discovered the measure of the shoot of the vault, or what proportion this shoot has to the weight of the entire vault. We only knew that this effort was very great, and opposed great masses of stones, or buttments, rather too strong, than too weak, but we did not know exactly what *medium* to observe.

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A B R I D G M E N T
O F T H E

PHILOSOPHICAL MEMOIRS of the ROYAL
ACADEMY of SCIENCES at *Paris*, for
the Year 1704.

I. *An observation of the quantity of rain
which fell at the observatory, with the
heights of the thermometer and barome-
ter, in the year 1703, by M. de la Hire **.

W H E N I undertook to make exact ob-
servations on the quantity of rain-wa-
ter which fell at the observatory in the course of
a year, I had no other view than to draw from
thence some knowledge of the original of springs,
on which I have made some remarks, and from
whence I have drawn considerable advantages for
the construction of cisterns, as I have related in
the memoir which I read to the publick assembly
of the academy, the 18th of *April*, 1703. But
as we are perswaded by most observations, that it
does not commonly rain but when the air be-
comes lighter, which is known by the falling
of the quicksilver in the tube of the barome-
ter; I thought that I ought to add to the ob-
servations of the rain, those of the barometer,
and relate at the same time the height of the ther-
mometer, to know what has been the degree of
heat or cold, when there has been more or less
plenty of rain. I have compared these different
heights of the thermometer, with those wherein

* Jan. 9, 1704.

it

it always remains in the bottom of the caves of the observatory, which I look upon as the mean heat and the mean state of the thermometer, filled with spirits of wine, which I make use of, and this height is 48 degrees.

This is the quantity of rain water which fell at the observatory, each month of the year 1703, which is measured by the height which it would have, if none of it was wasted or evaporated. I have already related in other memoirs of this kind, the manner in which I make these observations; therefore will not here repeat it. And tho' these observations have been made daily, I have thought it sufficient to give each month's result thereof, with some remarks on this subject, and principally of the winds which governed in *January*. The height of the rain water was 9 lines $\frac{1}{2}$, almost the whole of which fell near the beginning of the month, with a westerly wind, inclining sometimes to the S. and sometimes to the N. At the end of the month the wind was almost always to the N. and without rain.

In *February* there were 14 lines and $\frac{1}{4}$ of water; the wind was something changeable, but the greatest part of the month it was southerly.

In *March* it rained but 4 lines, tho' the wind was almost always towards the W. between the N. and the S.

In *April* there fell 16 lines and $\frac{1}{4}$ of water, distributed pretty equally throughout the whole month, the wind being almost always in the S. inclining to the W. and seldom towards the N.

In *May* I found 34 lines and $\frac{1}{4}$ of water; the governing wind was W. which sometimes changed to the S. but more commonly to the N.

In *June* there fell but 23 lines of water, the wind being almost always in the W.

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In *July* it rained 28 lines $\frac{1}{4}$, which fell at the beginning, in the middle, and at the end of the month. When it rained, the wind was almost always in the W. inclining to the S. and N. and in the intervals it was pretty often in the N. and a little in the E.

In *August* the rain furnished 23 lines $\frac{1}{2}$, of which 13 lines $\frac{1}{2}$ fell the 12th day of the month, beginning with a little storm, the wind being E. S. E. the wind was almost always in the N. and sometimes inclining to the E. and W.

In *September* the whole height of the rain water rose to 20 lines $\frac{1}{4}$, which was distributed thro' the whole month. The prevailing wind was S. W.

In *October* I got 17 lines of water, which fell each time in little quantities throughout the course of the month. The wind was almost always in the W. inclining to the S. and seldom in the N. or E.

In *November* I collected but 13 lines of water, which fell at the beginning and towards the end of the month, with a S. wind. From the 4th of the month to the 19th, it did not rain, the wind being almost always in the E. and sometimes to the N.

In *December* there fell but 3 lines and $\frac{1}{4}$ of water, but there were many fogs. In two thirds of the month, towards the end of it, there was no rain, tho' the wind was pretty often towards the W. except on the last days, when it was about the E.

The sum of the rain water of the whole year was 208 lines $\frac{1}{4}$, or 17 inches 4 lines $\frac{1}{4}$, which is a little less than the common, which is 19 inches; so that we may say, that this year is one of the dryest of these countries.

The

The four months of *May*, *June*, *July*, and *August*, gave more water than the other eight together, which commonly happens, tho' there have been no considerable storms.

The little snow which fell in the beginning of this year, is not worth being considered. We see thereby, that it is not the great quantity of snow which makes the earth more fruitful, as is commonly believed, since this year has produced much corn and fruit. It is true that snow, continuing long on the surface of the earth, may keep the salts therein, which throw themselves out of it continually, and which entering the earth again, when the snow melts, may make it more fruitful; but there may also be rains which may have the same effect, if they are impregnated with the same salts.

The cold was not considerable throughout the months of *January* and *February*, wherein it commonly is the greatest, for my thermometer did not fall to 26 degrees; and I have observed, that it does not begin to freeze till this thermometer is at 32 degrees; from whence also we may see, that there have been no considerable frosts. In the last months of this year the cold was not so great as at the beginning.

If the cold was not considerable this whole year, the heat was also but moderate, and of little continuance; and I find that the hottest days were the 27th of *May*, the last days of *July*, and the first of *August*, wherein the thermometer was near 60 degrees. The hottest day was the 12th of *August*, in which the thermometer rose to 64 degrees. These observations are always made near the sun-rising, which is the time wherein the air is coolest; and between two and three in the afternoon, the heat of the day is

greatest. I have observed during the summer, that between two and three in the afternoon, the thermometer rises 10 or 12 degrees more than in the morning at the time of the sun-rising, tho' this thermometer is always in the shade.

The 28th of *April*, the thermometer having been in the morning at 42 degrees, and the barometer at 27 inches 3 lines $\frac{1}{2}$, there was a storm and pretty loud thunder; and I was told, that near *Villeneuve St. George*, there fell on the earth a very great flame, which frightened those who lay near it, but which did no hurt to those who were in the place where it fell.

As to the barometer, it was at the highest on the 10th of *December* in the evening, at 28 inches, 4 lines and $\frac{1}{6}$, at the height of the great hall of the observatory, and in the whole month of *December*, the barometer always kept up very high; nor did it rain but 3 lines $\frac{1}{4}$, and this is a confirmation of what is generally observed, that it very seldom rains when the barometer is raised higher than its mean state. There also happened very near the same thing in *March*, when it did not rain more than 4 lines; but the barometer was not quite so high as in *December*.

The third of *January* the barometer was the lowest in the year at 26 inches, 6 lines $\frac{1}{4}$, with a little rain, and without storms, as it often happens when it is very low. Thus the difference between the greatest and the least height of the barometer, was this year 1 inch, 9 lines and $\frac{1}{2}$, which is a little more than the common, which does not exceed 1 inch, 6 lines.

I observed on the 18th of *December*, the declination of the needle from 9° 6' N towards the W. with a needle of 8 inches length. This needle is very well made, and is shut up in a wooden

wooden box of a square figure; and to make the observation, I always place the side of this box in one and the same part of a pillar, in the lower stone gallery of the observatory, whose position in the meridian line I am certain of, by very exact observations on the centre of the sun's passage in the meridian.

II. *New observations on the insects of orange-trees, by M. de la Hire* *.

I had formerly examined what is called the grain of cochineal, and I had given a memoir of it to the academy, in which I related at length, all the discovery I could make of them by their figure, after having soaked them; and among other things I had observed, that it was a little insect, of which nothing remained entire but the belly, which was covered with scales; but no part of the body near the head was to be seen, nor any feet, which I imagined had been dry'd up and reduced to dust.

It came then into my thoughts, whether the little insects on orange-trees were not the same with the cochineal; for the figure of the belly appeared to me much like them, and these insects feeding on the juice of the red fruits of *Opuntia*, where they gather cochineal, might give them the red colour and strong dye, with which they are filled.

I had often observed, that in bruising with the fingers the insects on orange-trees, they remained dyed of a reddish colour, which sticks pretty fast to the skin, tho' these animals feed only on the juice of green leaves and stalks of the tree; and this is what persuaded me that there was great likelihood in my conjectures; that if these

* March 8, 1704.

insects fed on the juice of the red fruits of the *Opuntia*, they might give a very strong red tincture; and what more confirmed it was, that I knew those who had eaten of these fruits, had made urine as red as blood.

To succeed in my design, as I had some plants of *Opuntia*, which had very red fruits upon them, I placed them underneath, and very near some orange-trees, whereon were many insects not yet hatched. I also broke several of the bags which contained the eggs, and spread a great quantity of them over all the *Opuntia*, hoping that some of these animals would fasten upon it. I observed every day, with a great deal of care, both the leaves and fruits of the *Opuntia*,; and at last I perceived one day a very great quantity of little white insects, which ran with great swiftness upon the *Opuntia*. I also looked on the orange-trees, and in like manner found them proportionably there. I then made no doubt but that these little insects were hatched from those eggs. A little time after, all these insects fixed on the orange-trees, round the branches and under the leaves, and they forsook the *opuntia*, on which there remained none of them, either on the leaves or fruits. Thus I concluded, that these insects of the orange-trees, tho' in appearance something like the cochineal, could not find on the *opuntia* a food suitable to them, as on many other plants, and that these were not the same.

However, my enquiry was not altogether useless to me; for I then knew that the insects on orange-trees, from their being hatched till a certain bigness, to which they arrived in a little time before they fixed themselves, might couple, and be in a condition of laying fruitful eggs, at a time far distant from that of their coupling, which is

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not till eight months after; and what seems yet more extraordinary, is the great growth of these insects after they are fixed and settled, till the time of their laying; for they become 20 or 30 times greater than they were before, and their outward figure being changed, they now appear only like a pretty long piece of tortoise-shell.

It were to be wished that some of the seeds of cochineal could be conveyed into the southern parts of *Europe*, as *Sicily* and *Spain*, where the *opuntia* may easily be brought; for I make no doubt but that cochineal may there be sufficiently cultivated, to know perfectly the nature of it, without being obliged to refer to the relation of clowns and slaves, who look on the productions of nature only with regard to the profit which it brings to them.

III. *That all the barometers, double as well as simple, which have been hitherto constructed, act not only by the greater or less weight of the air, but also by its greater or less heat; and the means of preventing this defect for the future in the construction of double barometers, and of correcting the error in the use of the simple barometers, by M. Amontons*.*

It is proper in the first place to relate the particulars of some experiments, in order to deduce afterwards from thence, if possible, a construction which may prevent the alteration caused by heat in the weight of quicksilver, with which common barometers are filled.

* June 18, 1704.

Experiment I.

The thermometers, of which mention is made at the end of the *connoissance de tems* of 1704, being at 54 inches, 5 lines, an areometer was filled with quicksilver, in which entered 18 ounces, 7 drams, 63 grains. After having emptied the areometer, it was filled with spirit of wine, and there entered 1 ounce, 1 dram, 28 grains. The quicksilver, spirit of wine, and thermometer had been a considerable time, as several days, in the same place near one another.

It follows from this experiment, that the weight of the quicksilver is to that of the spirit of wine in an equal mass, near as $16\frac{1}{4}$ to 1, when we do not try it either in great cold or great heat.

Experiment II.

The same thermometers being at 54 inches, 11 lines, a little common thermometer glass was filled full of quicksilver, the whole which entered therein was 757 grains: the bigness of the tube was such, that, in the length of 11 lines, it contained 18 grains. At this rate, a tube of like bigness, and of 36 inches, 6 lines $\frac{2}{3}$ long, would have contained the 757 grains of quicksilver. The thermometers being fallen to 50 inches, 11 lines, the little quicksilver thermometer fell just 2 lines; from whence we must conclude, that from the great heat to the great cold of our climate commonly taken, that is to say, in the time when my thermometers ran from 50 to 58 inches of their graduation, the bulk of the quicksilver augments by about $\frac{1}{11}$, part of what it had in the great cold, and that in equal bulks the weight of it also diminishes in great heat $\frac{1}{11}$, from that which it had in the great cold.

Expe-

Experiment III.

The thermometers being at 54 inches, some spirit of wine was put into a glass tube, sealed at one end: it occupied in this tube 32 inches, 4 lines in length; the other end of the tube was afterwards sealed, and it was left for the experiment. The thermometers being fallen to 50 inches, the spirit of wine in the tube was sunk 7 lines $\frac{1}{4}$; from whence it follows, that from the great cold to the great heat of our climate commonly taken, the bulk of the spirit of wine augments about $\frac{1}{7}$ of that which it had in the great cold.

It also follows from these three experiments, that in the great cold of our climate, the weight of the quicksilver is to that of the spirit of wine near as 16 to 1.

This being settled, if we suppose that in the great cold the space between the surfaces of the quicksilver of the two boxes of the double barometer is 28 inches, 8 lines; one of these inches will counter-balance, or be an *equilibrium* to 16 inches of spirit of wine, and the top of these 16 inches of spirit of wine, will mark for that time on its tube the place where the atmosphere equals the remaining 27 inches, 8 lines.

By taking the top and bottom of this point of the equal parts of 16 lines, each of these parts will be analogous to the lines of the quicksilver of the simple barometer, that is, the spirit of wine of the tube being at the first division under that which marks 27 inches, 8 lines, will shew that the air then weighs 27 inches, 9 lines, and only 27 inches, 7 lines, when the spirit of wine shall be at the first division above that which marks 28 inches, 8 lines. Mean while it must be observed that each of these parts of 16 lines ought

to be diminished $\frac{1}{8}$ of a line, if the aperture of the tube, which contains the spirit of wine, is half that of a line, and the diameter of the box an inch, the reason of which is, that the spirit of wine which enters into this tube cannot go out of the box, without making the quicksilver fall in a quantity which equals $\frac{1}{8}$ of a line; which makes a difference of $\frac{1}{9}$ of a line in the height of the quicksilver to each part, and there must be $\frac{1}{9}$ of spirit of wine to be an equilibrium to $\frac{1}{9}$ of quicksilver.

The cold supposed to be still the same, and the barometer being thus regulated, it is evident, that it will exactly mark all the changes which shall happen in the weight of the atmosphere, with this advantage over simple barometers, that it will mark them at least 14 times more sensibly: but in the great heats of our climate, these 28 inches, 8 lines of quicksilver, which in the great cold made an *equilibrium* with the weight of the atmosphere, will weigh $\frac{1}{11}$, less, and ought consequently in order to continue to counter-balance the same weight of air, to be augmented about 3 lines, which are very near the $\frac{1}{11}$, of 28 inches, 8 lines, without which the spirit of wine will sink in its tube 48 lines *minus* $\frac{1}{8}$ of a line, that is to say, a little more than 3 inches $\frac{3}{4}$.

This augmentation of 3 lines to the height of the column of quicksilver, cannot be made till the surface of the quicksilver of the inferior box falls a line and half, for then this line and half of quicksilver being driven into the superior box, will make one total height of quicksilver of 28 inches, 11 lines, between the surfaces of the quicksilver of the two boxes. Now it is necessary, in order to hinder this falling of the quicksilver in the inferior box, from causing any change

In the height of the spirit of wine in the tube, that the part of the spirit of wine, which is in the inferior box, should dilate itself sufficiently to fill this space of a line and half, which the quicksilver abandons; which will necessarily happen, if there is given to that part of the box which contains the spirit of wine, an equal capacity with that of a cylinder of the same diameter as the box, and of 40 lines $\frac{1}{2}$ of height, since these 40 lines $\frac{1}{2}$ contain 27 times 1 line and half, and the spirit of wine, by the third experiment above related, augments its bulk $\frac{1}{27}$ from great cold to great heat.

It now remains to consider the changes which heat may make on the spirit of wine contained in the tube, according as it may be at different heights, and as the degrees of heat vary.

First, it is now very certain, that while the weight of the atmosphere stops the spirit of wine at the bottom of the tube which contains it, whatever change happens to the heat of the air, the spirit of wine in the tube will not change its situation, and that the whole action of the rarefaction of the liquor, will be made on the side of the superior box.

It is also very evident, that in great cold, whatever height the spirit of wine may have in the tube which contains it, it will always mark precisely the augmentation or diminution of the weight of the atmosphere, since it is supposed to be in the state of great cold, that the barometer has been regulated.

It is then only the different heights of spirit of wine in the tube, except the time of great cold, which can make any alteration in the exactness of this barometer; and though this alteration in

the greatest heights of the spirit of wine, in times of the greatest heats, cannot at most exceed 14 lines, and that it is very inconsiderable at all other times, wherein the heat is less; yet this is the way to correct it, when it is necessary to be exact in the observations.

If * in the tube, which contains the spirit of wine, it rose therein thro' the little weight of the atmosphere, in the time of the great heats, to the height of 28 inches, there would then be on this height of 28 inches one inch to be corrected, because then these 28 inches would weigh no more than 27 inches, in the time of great cold. Therefore if they take this tube of 28 inches for one of the sides, about the right angle of a rectangular triangle, and at this height of 28 inches, a line is drawn of an inch perpendicular to the tube, which makes the other side of the right angle of the said triangle; this last side being divided in so many equal parts, as the thermometer contains degrees from winter to summer, and numbered in like manner, for example, in 8 with the same figures, and that from each of these parts the right lines are carried to the extremity of the other side, so that they divide the triangle into 8 equal triangles, there will be no more to do than from all the divisions of this first side to carry lines parallel to the second side: these parallels will each be divided into as many parts, as that by the lines carried from its divisions to the extremity of the first side, and all these divisions will be analogous to the degrees of the thermometer, and will indicate how the liquor should be corrected, that is to say, how much of its height should be diminished.

* Plate IV. Fig. 1.

Example

Example.

The thermometer being at 56 inches, the liquor in the barometer at 27, let there be taken from the height of the liquor a quantity equal to that part of the parallel 27, which is contained between the lines 50 A and 56 A, and thus of others.

As to the simple barometer, the whole extent of its march being bounded in a space too little, for such a scale as the preceding to be of any use to correct it in such a manner as is necessary, the following table may be used, which shews how much a column of quicksilver of 28 inches, 9 lines, will lengthen or diminish at all the degrees of heat indicated by my thermometer.

This increase or diminution is expressed in this table by $\frac{3}{2}$ of a line. Thus, for example, over-against 55 inches, 5 lines, is to be found 65, which means that at the time when my thermometers mark 55 inches, 5 lines, there must be diminished from the height of the quicksilver of the simple barometer an equal quantity to 2 lines $\frac{3}{2}$ of a line.

It should also here be noted, that though 28 inches, 9 lines, are not the mean height of the simple barometer, this height being more commonly 27 inches, 6 lines; yet nevertheless this table may very profitably be made use of without the fear of falling into any sensible error.

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A table of the heights of quicksilver, which must be added or taken from that of the simple barometer, according to the different degrees of heat indicated by my thermometer.

Degrees of the thermometer.		Heights to be corrected.	
49 inches	0 lines ———	add	12 32ds of a line.
49	1		11
49	2		10
49	3		9
49	4		8 or $\frac{1}{4}$ of a line.
49	5		7
49	6		6
49	7		5
49	8		4
49	9		3
49	10		2
49	11		1
50	0		0
50	1	take away	1
50	2		2
50	3		3
50	4		4
50	5		5
50	6		6
50	7		7
50	8		8 or $\frac{1}{4}$ of a line.
50	9		9
50	10		10
50	11		11
51	0		12
51	1		13
51	2		14
51	3		15
51	4		16 or $\frac{1}{2}$ a line.
51	5		17
51	6		18

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Degrees of the thermometer.

Heights to be corrected.

51 inches. 7 lines — take away 1932ds of a line.

51	8	20	
51	9	21	
51	10	22	
51	11	23	
52	0	24	or $\frac{1}{4}$ of a line
52	1	25	
52	2	26	
52	3	27	
52	4	28	
52	5	29	
52	6	30	
52	7	31	
52	8	32	or 1 line.
52	9	33	
52	10	34	
52	11	35	
53	0	36	
53	1	37	
53	2	38	
53	3	39	
53	4	40	or 1 line $\frac{1}{4}$
53	5	41	
53	6	42	
53	7	43	
53	8	44	
53	9	45	
53	10	46	
53	11	47	
54	0	48	or 1 line $\frac{1}{4}$
54	1	49	
54	2	50	
54	3	51	
54	4	52	
54	5	53	
54	6	54	
54	7	55	

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Degrees of the thermometer.

Heights to be corrected.

54 inches. 8 lines — take away 56 32ds or 1 lin. $\frac{1}{2}$

54	9		57
54	10		58
54	11		59
55	0		60
55	1		61
55	2		62
55	3		63
55	4		64 or 2 lines.
55	5		65
55	6		66
55	7		67
55	8		68
55	9		69
55	10		70
55	11		71
56	0		72 or 2 lines $\frac{1}{4}$.
56	1		73
56	2		74
56	3		75
56	4		76
56	5		77
56	6		78
56	7		79
56	8		80 or 2 lines $\frac{1}{2}$.
56	9		81
56	10		82
56	11		83
57	0		84
57	1		85
57	2		86
57	3		87
57	4		88 or 2 lines $\frac{1}{4}$.
57	5		89
57	6		90
57	7		91

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Degrees of the thermometer.		Heights to be corrected.	
57 inches.	8 lines — take away	92	32ds of a lin.
57 —	9 —	93	
57 —	10 —	94	
57 —	11 —	95	
58 —	0 —	96	or 3 lines.
58 —	1 —	97	
58 —	2 —	98	
58 —	3 —	99	
58 —	4 —	100	
58 —	5 —	101	
58 —	6 —	102	
58 —	7 —	103	
58 —	8 —	104	or 3 lines $\frac{1}{4}$.
58 —	9 —	105	
58 —	10 —	106	
58 —	11 —	107	
59 —	0 —	108	or 3 lines $\frac{1}{2}$.

IV. *The history of the Formica-leo, by M. Poupart **, translated by Mr. Chambers.

The *formica-leo* † is an insect which resembles the spider, both in its disposition, the figure, and softness of its body, and its manner of spinning; it also bears some relation to the palmer, or *millepes*, and at first sight, one would even mistake it for that animal; 'tis of a dirty brownish colour, and beset with black specks, which when viewed thro' a magnifier, appear like so many prickles or quills of a porcupine. Its body is invested with *annuli*, which render it all wrinkled; it has 6 feet, 4 of them fastened to its *thorax*, and 2 to a long projecture, which may be taken for its neck; its head is very small and

* Aug. 30, 1704.

† Plate IV. Fig. 2 and 3.

flat, its 2 horns hard and hollow, about 2 lines long, and somewhat thicker than a hair, being hooked at the end like a cat's claws; by the microscope they appear much like the horns of a large beetle, called the *furvus bolens* *; at the base, or bottom of each is a little black eye, which is very sharp sighted, as appears from the animal's taking the alarm, and flying at the least appearance of an object.

It is called the *formica-leo*, q. d. *ant-lion*, from its living chiefly upon ants, which have the fortune to fall into its ambuscade; tho' this should hardly intitle it to the denomination of a lion; its artifice is rather that of a fox, and it would be more proper to call it *formica-vulpes*. The creature is obliged to be very grave and wary, as having nothing to live on but a few insects, which fall into its clutches, but especially ants, for all the little animals with wings escape its attempts, and most of the rest are either too big or too hard skinned to be penetrated and fastened with its horns.

Its way of catching insects is as follows, it usually encamps under the foot of an old wall, to be sheltered from the rain, and the place that it pitches on must be a fine dry sand, that it may be able to make a pit or trench therein, of the form of a tunnel.

To dig only a small pit, it bends its hind-part, which terminates in a point downwards, and with this, as a plough-share, opens the ground, walking backwards; and by little jirks, when arrived at a certain depth, it throws up the sand a great height with its head, and its ditch is finished; but to make a deep ditch, it first draws a large

* So it is written in Mr. Chambers's manuscript, it should be *cervus volans*, or flying stag.

circle, which is to be the top of the tunnel * ; then it digs under the sand, at each step it makes, which is always backwards ; in its descent, it describes a spiral line, which at length terminates in the point or tip of the tunnel or hollow cone.

† Its head is very fit to heave off the sand, as being flat and its neck very long, when not drawn in, by which means it is enabled to give violent tosses ; and accordingly I have seen it throw the *animalcula* it has sucked, to the distance of $\frac{1}{2}$ a foot from its den.

The pit being finished, the animal places itself aside of its bottom, shewing nothing but its two horns, which protend into the tip or bottom of the pit ; lying thus perdue, if an ant or other like insect happen to pass by the edge of the pit, it necessarily tumbles some of the loose sand into the bottom, which is a signal to the *formica-leo* that there is prey at hand ; upon this it falls a throwing up sand with its head upon the poor ant, in order to bring him tumbling down into the bottom of the cave between his horns, for the creature never runs after its game ; and if it happen that the ant do not fall plum between his horns at first, but that perceiving the snare laid for her, she begins to clamber out of the pit, the mobility of the sand sometimes lets her drop again ; or if this fail, the *formica-leo* being still upon the watch, throws up new sand to bring her down a second time ; and if she now fall between his horns, he gripes her fast, plunging them deep into her body, and proceeds to suck her so long as any moisture remains ; this done, the carcase is thrown out, and if any damage had been done to the pit, the animal makes it up again for a second chase.

* Fig. 5.

† Fig. 4.

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The *formica-leo* would rather die of hunger, than go seek its living like other insects, nor is it out of laziness as might be supposed, that it plays this fox's game, for it has no other, being unable to walk except backwards, and by little jirks. It lies night and day upon the scout under the sand at the bottom of its den, for that being unable to go out in quest of its game, it can only wait till chance brings it home to it, which happens very seldom, so that it is obliged to supply with perseverance, what nature has denied it by agility.

Indeed the reasons I am going to produce, seem to make all these stratagems of little effect to the subsistence of the animal, which one would imagine only caught insects out of diversion and wantonness, like the sportsman who only goes to the chace for the pleasure of pursuing for.

First, he only gripes the insects with the extremity of his horns, which do not seem to be perforated at the end, so that 'tis hard to conceive how he should suck their juices by that part.

Secondly, upon viewing him with a magnifier, there is no sign of his thrusting out a proboscis to suck the little animals he catches, as other insects do; and there is always a considerable distance between his head and the unhappy animal, which never comes nearer him than the points of his horns.

Thirdly, several *formica-leo's* have been shut up in a close box for the space of six months, yet have lived as well as others which were supplied with flies, and even made their pits, and overwent all the natural changes hereafter mentioned, which seems to intimate that the *formica-leo* may live without food.

But

But if it be considered, that its horns grow again after they have been cut ; that it dwindles and grows less when it has no food ; that after catching a single insect it appears visibly bigger than before ; and that by sucking a fly two or three hours, it renders it so dry and friable, that it may be crumbled into powder between the fingers. It must be allowed, that altho' the animal may live without our perceiving how, or by what part, it takes in its food ; yet food it does take.

I am persuaded, therefore, that its horns are to be conceived as two syringes, wherewith it pumps the juice of animals ; in effect, viewing them with a microscope, which magnifies a great deal, I observed a transparent, membranous body, running the whole length of the cavity of the horn, which may well enough serve for the piston, or sucker of the syringe.

The *formica-leo* being arrived at a certain age, and preparing for a renovation, in order to appear under a new form, it ceases to make any more pits, but continues to plough the sand, which is found all furrowed over with irregular tracks.

* After ploughing a long time, it nestles under the sand, where it makes a hollow ball, and shuts itself therein while it changes its figure ; this ball consists of silk, which it spins from its hind-part, much as spiders do of a *gluten* or viscid matter, issuing from all parts of its body, and of a sand which it finds in the place.

To make the ball, it turns slowly around as on a centre, vibrating its hind-part to the right and left, and making it touch the sand, that the silk may stick thereto, either by its catching

* Fig. 6.

on

on the inequalities of the grains of sand, or adhering to the glutinous matter it may be smeared with; but however this be, the sands stick so close to the silk, that 'tis difficult separating them, even though they be vigorously shaken while the work is yet soft.

This silk is incomparably finer than the common silk, being scarce perceivable without the help of a microscope; to observe it well, you must take it up ere the work be quite finished, in which case it will be found soft and cottony, as not being yet hardened by the *gluten*, which oozes at a very slow rate from the pores of the animal. Upon taking up this silk with the point of a needle, one sees a space between the grains of sand, but without seeing the silk that connects them, unless a magnifier be applied.

'Tis impossible, without a stratagem, to see how these little animals spin their silk, and build their cells, by reason they always work under the sand. In order to this, their work is to be taken up once, and again ere it be finished; upon which they begin again, till at length they are grown so feeble, that being no longer able to hide themselves under the sand as usual, they begin slowly to spin their silk on the surface thereof, after the manner above-mentioned.

The *formica leo* having wrought a long time, finds itself in the middle of a large soft ball, which hitherto only consists of silk and sand intermixed; but it hardens by little and little, in proportion as it imbibes the viscous matter oozing from the body of the animal, which penetrates the cell on all sides.

What chiefly convinced me of a viscid moisture transuding from the body of this animal is,
that

that there were several grains of sand sticking on the neck of one of my *formica leo's*, and forming a little kind of rock thereon; while this clog was on its neck, it ceased working at its pit, as being obstructed in the motion of its head; but upon breaking it with nippers, the animal immediately went to work.

When the *formica leo* is inclosed in its cell, it lines the inside thereof with new pure silk, unmixed with sand, forming a close kind of pearl coloured web, wherein the animal lies at ease, with its head between its legs. At first sight, one would take this web for a dry *gluten*, separated from the body of the animal; but if this were so, it would break with bending and folding, which it does not; add, that this lining is a continuation of the cell, or at least is fastened so close thereto, that there is no parting them without destroying the ball. Steeping some of this silk several days in water, I found it did not dissolve as a *gluten* would have done; tho' it lost its fine colour; an argument that the little *gluten*, which was mixed with the silk, and perhaps gave it its colour, had dissolved, and left the silk alone. This silk is much like that which certain spiders hang on the leaves of trees, to serve them as a lodge or *nidus*, to lay their eggs in, but is thicker than such spider silk.

That the *formica-leo* does not begin to line in the inside of its cell, till after it is finished, appears hence, that opening it before it be hardened, we find none of the lining above-mentioned.

My *formica leo's* remained 6 weeks or 2 months, in their cells, before they changed into maggots, tho' this time of their continuance is by no means regular and certain; they were disposed with the head between the legs, for the sake of roundness,
and

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and that they might take up the less room, and accommodate themselves to the concave figure of the ball as much as possible.

When the time was come for changing their figure, they began to put off their first skin, to which the horns, eyes, and hair were fastened. This skin was now like a shriveled clue, whitish on the inside, having an aperture all along the belly, at which the insect hereafter-mentioned had issued.

The *formica-leo* having quitted its skin, appears under the form of a maggot*, with 4 membranous wings, 6 feet, 2 large horns or hollow, soft *antennæ*, 2 black eyes, and 2 pinchers in form of saws, which serve it for teeth; the whole being about a quarter of an inch long. This maggot remains sometimes in its retirement e're it comes forth in a new form, tho' the precise time is not so easy to determine, by reason the *formica-leo* it springs from lies hid before the metamorphosis.

When the maggot is disposed to quit its cell, it eats a little roundish hole therein, which however does not appear round, by reason the piece usually remains fast on one side†, which renders the passage so narrow, that half the maggot remains in the cell, and the other half out. In this state, the maggot is no living thing, being only a transparent membranous case, with horns or *antennæ*, eyes, teeth, wings, feet, &c. which are the cases of the like parts of a beautiful fly, called a *maid*‡, which issues hence by a cleft made on its back near the head.

* Fig. 7, 8, and 9. † Fig. 10.

‡ So Mr. *Chambers* translates it: the original is *demoiselle*; it is an insect, called, in *English*, *adder-bolt*, or *dragon-fly*; see Vol. I. pag. 94, of this abridgment.

This fly is 15 or 16 lines long ; its wings at first are only 2 lines, by reason having been set in cases or frames of that length, they have assumed the figure and size thereof ; but we find them folded in 2 plaits, which, in two minutes time, open and stretch out, and thus grow longer than the body ; after quitting its case the *maid* remains sometime on its feet motionless, to let its wings dry, that it may take its flight ||, and enjoy a happier state of life than that it led under the skin of the poor *formica-leo*.

While the *maid* is inclosed in its maggot, it cannot attain to above 3 lines long, by reason the maggot itself is no bigger ; but it is no sooner at liberty, than it lengthens and extends to upwards of 15 lines. This sudden expansion arises hence, that the *maid*, while in its case is folded and contracted like a cat-cawl, when squeezed at the two ends ; but after its delivery hence, it recovers all its magnitude like a sponge, which having been crowded between the fingers, resumes its dimensions when let go again.

In the year 1703, the *formica-leos* I had observed did not change into *maids*, it being the year following ere that metamorphosis took place : an indication that these animals do not change in the first year, and that they must be to a certain age ere they be fit for transformation.

When the *maid* is gone, opening the cell the *formica-leo* had lain in, we find it lined as above-mentioned, with a glossy pearl coloured fatten, the shriveled skin or *exuvia* of the *formica-leo*, is always found therein, together with the membranous case which immediately inclosed the *maid* ; but what is more extraordinary, we

|| Fig. 11.

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sometimes find an egg *, which the *maid* deposits there, before it goes. The egg is 2 lines long, 1 thick, and bears some resemblance to a slender acorn; its shell is hard, and in all respects like that of a hen's egg. The substance contained in it is not fluid, and I have observed the eggs to be of different colours at different times; one of them being exposed some days to a hot sun, the matter within it grew hard and black as ink.

These young *maids* seem only to lay one egg, for we found but one a-piece in the bodies of several which had been opened, only one in a cell which another had relinquished, and another *maid* rising to the top of the box, it was kept in, some hours after dropped another; and yet 'tis scarce probable, that each *maid* should only lay a single egg, by reason there will always be some which are not prolifick, and others of them must produce males, from whence it is easy to infer, that the species by degrees must have totally ceased. The precipitancy wherewith these *maids* lay their eggs are a proof, that they do not always wait for the embraces of the male ere they bring them forth; and it may be owing to the scarceness of such coupling, that the *formica-leos*, and the little *maids* arising from them, are so rare.

The little ball the *formica leo* is lodged in, is absolutely necessary for the production of the *maid*; for breaking some of them, and turning out the *formica leos* naked upon the sand, they were ready for transformation. They did indeed put off their skin; but the *maids* could not get forth from the maggots they were inclosed in, tho' they lived a long time after, and made several attempts to get forth. One of the chief uses

* Fig. 12.

of this ball is, that by means hereof the *maid* slips off the maggot it is invested with, in squeezing, with some difficulty, through the little hole the same maggot had made with its teeth.

It must be observed, that the several kinds of *maids* frisking in the summer-time about rivers, bushes, &c. do not all arise from this little animal; such as do, have two *antennæ*, which are very slender towards the head, and grow bigger and bigger to the end. They have two large eyes on the sides of their head, and none atop of it, as the other *maids* have. Their belly is not furrowed lengthwise, as is found in the rest; and the tip of their tail is beset with hairs. Their wings are of an ash colour, spotted with black, without any vivid colours variegating them: so that 'tis probable those beautiful flies, which the variety of their colours has occasioned to be denominated *maids*, with all the several species thereof, are of another origin.

There are two other beautiful species of large *maids*, whose origin is certainly very different from those above-mentioned: they arise from two water animals, which have no resemblance to the *formica-leo*.

We shall take occasion hereafter to shew, that the animals they arise from are real fishes; for we have observed their gills; and shall here, by the way, present the figures thereof*, as well as of the animals themselves†.

Explanation of the figures, translated by J. M.

Plate IV. Fig. 2. This figure represents the *formica-leo*, drawn three times as big as the life, to show how he is bristled with prickles.

3. The under part of the *formica-leo*.

* Fig. 15 and 16.

† Fig. 13, 14, and 17.

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4. The head and neck of the *formica-leo* separated from the breast, and drawn much larger than the life, that the smallest parts may be distinctly seen.

5. The pit which the *formica-leo* has made for insects to fall into. He is hid at the bottom, where he lets nothing but his horns be seen, which he keeps divided, that he may be quite ready to seize the little animals.

6. The lodge in which the *formica-leo* is shut up, in order to change its form.

7. The little worm which appears after the *formica-leo* has quitted its skin, in which the *demoiselle*, or *adder-bolt* (11) is inclosed.

8. This figure represents the little worm (7) drawn much larger than the life, that its eyes, feet, and wings may be distinctly seen, which are sheaths or cases, in which the same parts of the *demoiselle* are contained.

9. This odd figure, which is drawn much larger than the life, is the little worm which was represented in the figures 7 and 8, in its situation in the lodge. Its back is bent, that it may suit the figure of his lodge, and take up less room.

10. The ball, or lodge, of the *formica-leo*, with the worm marked 7, part of which is within and part without; from whence the *demoiselle* (11) is come out through a crevice made on the back of the worm.

11. This figure represents the *demoiselle*, when she is come out of the worm 7, 8, or 9.

12. The eggs which the *demoiselles* lay almost as soon as they are come out of their little lodges, or balls.

13. An aquatic animal, from whence proceeds a great kind of *demoiselle*, different from that which comes from the *formica-leo*. This little animal is a real fish.

14.

14. The underpart of the aquatic animal represented in *fig. 13*.

15. A sort of mask, which covers the head of the aquatic animal marked 13, which are the gills seen outwardly.

16. A mask which covers the fore part of the head of the aquatic animal marked 13, which is its gills seen inwardly.

17. Another aquatic animal, a little differing from the preceding, from which comes out a great kind of *demoiselle*, variegated with fine colours.

V. *A discourse on the barometer, by M. Amontons* *.

Among the physical discoveries of the last age, that of the barometer, or of the manner of measuring the weight of the atmosphere, may well hold the first place.

The clearness and evidence with which we now explain many effects of nature, where we only saw obscurity and uncertainty before this discovery, are convincing proofs of it. Hardly any body is ignorant that the effects, which were formerly attributed to the abhorrence of a *vacuum*, had causes which were wholly unknown to those very persons, who were most fond of using this expression.

Thus, what is very obscure and impenetrable at one time, becomes the utmost evidence in another.

But altho' it be true, that since this discovery, we have cleared upon this subject an infinity of very difficult things, with all the clearness that can be wished; we cannot however doubt, but that there yet remains a great number, and that these last are much more so, as they are less ap-

* Nov. 12, 1704.

parent, and that they do not present themselves at first to the mind, as the former did.

In the novelty of the barometer, the surprizing effects of the weight of the air alone, have drawn all the attention of those who saw them, we suffered ourselves willingly to be prepossessed, that it was the sole cause of the motion of the quicksilver; and if we reflected, that there is nothing upon which heat does not act, we thought that in this case it was so little, that it was not worth regarding. We easily passed over a reasoning which had nothing new, to admire a system, of which the novelty agreeably surprized us by its happy success; and we had, if I may so say, eyes only to consider a multitude of experiments, all of them curious, which presented and explained themselves, as of their own accord, without there being need to determine any thing exactly.

In effect, it signified little to give the reason, for example, of pumps, siphons, and almost all the other experiments of the gravity of the air, to know that the weight of the quicksilver was not the same in summer, as in winter. It was sufficient, that we were assured that it was not so considerable a quantity as to hinder the determining in general the elevation of the quicksilver in the tubes to about 28 inches, and that of water to 32 feet.

But, in fine, these apparent and palpable effects of the weight of the atmosphere being now sufficiently explained in a general manner, it remains for us to do it in a more particular and exact manner, and to carry our attention to others, which by being more concealed are not less useful.

The only thing here, that could trouble us, is, that the barometer proper for explaining in general

ral the effect of pumps and siphons, becomes faulty and bad when it is used, for example, to measure the vicissitude of the weight of the atmosphere, to determine the height of it, and to level many points of the surface of the earth. In the observation of the greater or less gravity of the atmosphere, we may find a difference of above 3 lines in the height of the quicksilver, tho' in reality the weight of the atmosphere has not changed; which proceeds from the effect which the heat produces upon the quicksilver of the barometer, experience having taught us, that a column of quicksilver of 28 inches, 9 lines in winter, and one of 29 inches in summer, are of the same weight; so that the simple barometer being carried in the coldest time of our climate, from a place above the surface of the earth, into another made hollow beneath it, may give 1 line $\frac{1}{2}$ difference in the height of the quicksilver, which we have falsely attributed to the weight of the column of air which was between these two places; and if we had a mind to determine by this experiment the height of the atmosphere, or the difference of the level of two places of the earth, we should run great risque, of doing both very badly.

The barometers are either simple, that is, charged only with quicksilver; or double, that is, besides quicksilver a 2d liquor is also used, which is commonly oil of tartar coloured. As for the simple barometers, the extent of their motion is very moderate, hardly exceeding 23 or 24 lines, and there is no other way to avoid errors in these, than to prepare a table of correction, which shews the proportional quantities by which the heat lengthens the column of quicksilver, from winter to summer, and that consequently it is convenient

venient to retrench from the heights indicated by the barometer, at the time of the observation. For example, my thermometers, that is, those which are described at the end of the *connoissance des Tems de 1704*, and in the memoirs of 1702 and 1703; these thermometers, I say, marking 58 inches, which is the time of our greatest heats, there are 3 lines to be retrenched from the height of the quicksilver of the simple barometer; 2 lines when these thermometers mark 55 inches 4 lines; only 1 line when they mark but 52 inches 8 lines; and 0, or nothing, when they mark but 50 inches; and so of the other corrections to be made for all the other degrees of heat between these, which we shall find by preparing an exact table upon this foundation.

But as to the double barometers, whose motions are much more considerable, and upon which the heat produces different effects, the combination of which is the cause that we cannot easily make the corrections by a table; besides that the persons, who make use of these barometers, are for the most part but little accustomed to these sort of corrections; this is the way that I have used, that these corrections might be made as it were of themselves, and without a table.

These barometers are composed of two glass boxes * A B, which have a communication with one another by a bent tube A C B.

The box A terminates in a point which is hermetically sealed. The upper half of this box is emptied of gross air, the other half, the tube A C B, and the lower half of the box B, contains quicksilver. This ball B terminates in a very slender tube B D, open at D. The upper half of the box B, and part of the tube B D, con-

* Plate IV. Fig. 18.

tain a liquor which rises and falls in the tube, according as the atmosphere is more or less light, the quicksilver A C counter-balancing and making always *equilibrium* with the quicksilver CB, the liquor B D and the atmosphere.

All this at present seems to be almost universally known, but that which seems not to have been yet observed by any body, is, that the quicksilver contained in A B, becoming lighter in the summer than in the winter, the atmosphere repels toward the bottom the liquor contained in the tube B D, to 3 or 4 inches, and makes us falsely presume that the atmosphere is become thus much heavier, tho' in reality its weight has not been at all changed.

Therefore to prevent this fault, the column of quicksilver A B must be lengthened sufficiently, to replace the weight which the heat makes them lose, without the liquor in the tube changing its place. For this purpose I took a liquor, which easily rarefies by the heat, as spirit of wine does; I substituted this liquor to oil of tartar, which is commonly used, and which does not rarefy near so sensibly.

I increased the capacity of the box B, which commonly contains this liquor, that it might hold more, and also produce a sufficient rarefaction, to make the quicksilver of the box fall, and by this means lengthen the column of quicksilver A B, which without that would not lengthen at all, altho' the heat had rendered it lighter, because the atmosphere not pressing immediately upon the quicksilver of the box B, but upon the liquor in the tube D, it would make this liquor sink and supply by this means the lightness of the quicksilver; which, as I have already said, would make us falsely presume that the atmosphere is become hea-

vier, altho' it is not at all changed; whereas the liquor in the box B, finding in its rarefaction always the same resistance on the side of the atmosphere, supposing that its weight has not changed at all; and finding less from the side of the quicksilver rendered lighter by the heat, this liquor employs the whole action of its rarefaction against the quicksilver, which it repels and replaces always *in equilibrio* with the atmosphere, without the liquor of the tube D, upon which the atmosphere immediately acts, being constrained to change place, unless the atmosphere changes its weight; and the whole artifice of this consists only in well proportioning the capacity which contains the liquor to the capacity of the tube of the barometer; for too small a one would not entirely correct the error; and too great a one, by repelling the quicksilver too much, would make the liquor in the rarefaction find at last too much resistance from the quicksilver, and be obliged to act on the side of the atmosphere; which would make us falsely presume that the air was become lighter.

It remains for me to observe, that altho' by this means the error that might happen by the greater or less lightness of quicksilver is corrected of itself; there is also a second to be avoided, which might be caused by the greater or less lightness of the liquor.

This error is truly not so considerable as the first, and might very well be neglected without any great consequence; but it will be always better to have regard to it, chiefly in cases where exactness is required; and this might be done by the means of graduation, as I am going to say.

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We commonly divide this graduation into parts equal among themselves, which signify nothing, and are only to express by their greater or less number, that the liquor is more or less high, and consequently that the atmosphere is more or less light, but not how much; and these numbers never express the weight of the atmosphere.

I have therefore judged it most proper that these parts, altho' much greater, should represent the inches and lines that the quicksilver reaches in the simple barometer, from the least to the greatest lightness of the atmosphere.

Thus I divide all my graduation, which is about 28 inches, into 24 equal parts, which express the 24 lines comprehended in the simple barometer between 28 inches 4 lines, which is the greatest gravity that I have known in the atmosphere, and 26 inches 4 lines, which is the least.

I allow to this graduation about 14 lines in breadth at the top, and only a line and $\frac{1}{4}$ or thereabouts at the bottom.

I divide each of these breadths into 8 equal parts, and draw right lines from the divisions at the top to those at bottom, which make 8 *trapeziums* about 28 inches long.

Lastly, I cut all these *trapeziums* by lines parallel to each other, drawn from the 24 ascending divisions, after having numbered these 24 divisions, in descending from 26 inches 4 lines, to 28 inches 4 lines, I numbered the 8 lateral divisions from 50 to 58.

These 8 lateral divisions represent the 8 inches contained upon the graduation of my thermometer, from 50 to 58, that is, from the greatest cold to the greatest heat of our climate, and serve to make the correction of the error, that the greatest or least lightness of the liquor of the barometer

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meter might cause, and that in the following
manner.

I first consider at what division my thermometer is ; I afterwards take upon my barometer over-against the place where it is found, the lateral part contained between the first ascending line of the graduation, and the ascending line which answers to the division that I have observed upon the thermometer.

Adding this part to the height of the quicksilver that the barometer indicates, I have the exact weight of the atmosphere.

Here would be the place to give the reason of the particular construction of this barometer, and of its graduation ; but as it is deduced from a detail which would be tedious, and which I have already given * ; those who would know more of it, may have recourse to it. I shall content myself with observing that this barometer, besides its great exactness, has also the advantage of being almost as sensible again as the others, and that great care must be taken that there does not remain any air in the top of the upper box under the quicksilver.

After what I have just said of the effect of the heat upon the liquors that the double barometers are filled with ; it remains to examine what its action may be upon the glass that contains these liquors ; and if there is no room to fear that this may alter the indication also of the greater or less gravity of the atmosphere, which is not without foundation. For, in short, we know nothing in nature, of all that falls under the senses, upon which heat does not manifest its power ; thus there is no doubt of its

* Pag. 161 of this volume.

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acting upon the glass, as upon all other things, and of its dilatating it in such a manner, that the capacity of a glass vessel or bottle is greater in the summer than in the winter. But the question is, to know if this may be considerable enough to cause any alteration in the barometer.

Now by many exact experiments, I have found that a white glass bottle, pretty thick, of a cylindrical figure, and such as those are, which are commonly stopped with a glass stopper, full of common water, of which the degree of heat measured by my thermometer was equal to 54 inches, and which contained about 14 ounces of this water, increased its capacity only $2\frac{1}{688}$, when I immersed it in other water, of which the degree of heat measured upon the same thermometer, was 64 inches: from whence we may judge, that this effect is so trifling, that it cannot be sensible in the glass of a barometer, the capacity of which is nothing near so considerable, as that of this bottle.

VI. *Extract of a letter from M. Sarrafin *, physician to the king, in Canada, concerning the anatomy of the castor, read to the academy, by M. Pitton Tournefort; translated by Mr. Chambers.*

The largest castors are 3 or 4 feet long, and 12 or 15 inches broad in the middle of the *thorax*, and from one haunch to the other; and they are commonly from 40 to 60 *lb.* weight. Their life hardly lasts above 15 or 20 years; as to colour, they are usually very black in the far-

* From Quebec, Oct. 15, 1700.

these parts of the north, tho' sometimes also white; those of *Canada* are generally brown, but this colour grows brighter as the countries are more temperate, for they are fallow; and even almost straw-coloured among the *Illinois* and *Chaouanons*.

The castor, whose description we here give, was very black, tho' taken on the edge of a little lake 12 or 15 leagues from *Quebec*; it weighed about 50 pounds.

This animal is every where beset with 2 kinds of hair, except on the legs, where there is but one, and that very short hair; the first kind of hair is from $\frac{1}{4}$ of an inch to two inches long, being continually shorter; the nearer it is to the head and tail, it is the thicker, rougher, and more glossy kind, being that to which the colour of the castor is chiefly owing; viewing it with a microscope, we find a line in the middle more transparent than the rest, which makes us conjecture it to be hollow.

The other kind of hair is a very fine close down, about an inch long, which helps to keep the castor from the cold, and serves to make hats and other stuffs withal; those skins which have been wore by the savages, either as garments or as coverings of beds, are the most valued, both as they are stripped of their coarse hair, and as the down left on them being become fat and unctuous, by the matter of insensible perspiration, works so much the better; this down, when the animal is alive and works, is preserved from the mud and dirt, by the other longer and stiffer kind of hair.

'Tis not easy distinguishing at first sight whether a castor be male or female; all that appears without is an orifice under the tail, whereby both kinds

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kinds of excrements are evacuated. The parts which distinguish the sex are hid under the muscles, and to make a judgment thereof, the skin and flesh between the *os pubis* and this orifice, must be taken up, upon which the yard will be felt, which is hard and about the length and thickness of a finger.

Under the skin we find a layer of fat, which under the belly is 8 or 10 lines thick, and extends from the jaws to the tail, but it diminishes gradually in approaching towards the back where there is none at all. A second layer of fat we find between the two oblique muscles of the belly, but this is hardly $\frac{1}{4}$ of an inch thick, the viscera have scarce any, and the caul, tho' as large as in other animals, only weighs 3 or 4 ounces.

The muscles of the castor are all very strong, and seem bigger than needs be, considering the size of the animal. The fibres of the cutaneous muscle have very different directions; those covering the back, from the hips to the neck, are strait, and so big, that the muscle is here almost an inch thick; the fibres situate aside of these, recede gradually from hence, and make a much less bulk; they describe almost semi-circles, which descending on the pectoral muscles, the *sternum*, and all along the strait muscles, unite by an *aponeurosis*, in such manner, that they invest the whole animal. Part of these fibres cover the hips, after which traversing each other on the *os pubis*, they descend and form a kind of tissue in manner of a mat, which covers not only a very considerable bundle of fibres, but also the *sphincter* of the *anus*.

From the inner surface of the mat abovementioned, about 12 or 15 lines below the *os pubis*, arise two branches of fleshy fibres, about the thick-
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ness of a finger, which ascend to the insertion of the strait muscles, and are fastened thereto. Of that part of this muscle which covers the back, and whose fibres are strait, a strong *aponeurosis* is formed, which invests all below the thighs; it is fastened to the *spinæ apophyses* of the *vertebræ* towards the tail, and from space to space adheres likewise to the membranes of the muscles, which give it motion.

The same plan of fibres, being arrived at the first *vertebræ* of the back, divides first into two parts, which form several heads, and by different origins are inserted into different parts; there is one about 2 inches broad, which rises to the third *vertebræ* of the neck, and is fastened upon the *rhomboides*; another fixes itself upon the head of the *omoplate*; the third on the hind and inner part of the arm, the elbow, and the upper and hind part of the first joint of the arm; lastly, the fourth makes one and the same tendon with that of the first and broadest, and of this is formed a fifth, which is inserted into the middle and lower part of the first joint of the arm.

There is nothing particular in the muscles of the belly, except that the little oblique muscle and the *transversalis* are inseparable.

The castor's liver is of a ruddy brown colour, and divided into 7 lobes, which possess the 2 *hypochondria*, and thus cover the stomach on all sides. The gall-bladder is fastened to the largest of these lobes, and usually discharges itself into the *duodenum*, tho' M. *Sarrafin* has met with one which emptied itself into the *jejunum*.

The spleen is round and not above 4 lines in diameter, to 3 inches long; it is more firm than that of other animals, and is fastened by 5 or 6 very short vesicles to the fund of the stomach,

as also by certain membranes to the kidneys, *pancreas*, and *colon*. A few conglobate glands about the bigness of bees are also found towards that end of it next the stomach, which is somewhat bigger than the other.

The kidneys are $\frac{1}{2}$ an inch thick, 2 inches long, and about as many broad; the renal glands are under $\frac{1}{2}$ an inch in length.

The *pancreas* is at least 2 foot long, and forms an angle, the point whereof is fastened to the large lobe of the liver, by a sort of little threads; it is divided into two parts, one whereof passes under the stomach, and is fastened to the spleen and the left kidney; the other descending along the *duodenum* and *jejunum*, open into the same by several little passages.

The *oesophagus* is wholly invested with a white membrane, which appears like a kind of lining, and may easily be parted from the canal without tearing it.

The stomach is one of the most singular parts in this animal; it is upwards of a foot long, and 4 inches broad towards the spleen, but it lessens by degrees, so that after about two thirds, contracts a full $\frac{1}{2}$ by a projecture upwards of an inch, which advances into its cavity; after this it widens about 3 inches towards the *pylorus*, which is considerably prominent, and advances towards the spleen by a membrane fastened to the *oesophagus* at its other end; the widening above-mentioned seems to make a second stomach, tho' its real use be no other than to retain the food a longer time, and especially the more solid kinds, as wood, which here only yields a very slight extract, for it passes almost as it is swallowed down, whereas herbs, fruits, and roots, undergo an entire dissolution.

The membranes of the stomach are so thin, that the part is apt to tear upon the least swelling, tho' the fleshy membrane thickens on the side of the *pyloras*, and fortifies it. We meet with no glands dispersed in this stomach, but in lieu thereof it is beset with about 100 vesicles or little bladders, 2 or 3 lines long, which are fastened on the nervous membrane, and cover'd by the fleshy one, their situation being between the strait part of the vesicle and the *oesophagus*; all these vesicles makes a kind of hemispherical body, 7 or 8 lines high, and about 3 inches broad at bottom; the inside of each vesicle seems glandulous, but they are so delicate that they burst upon the least pressing them, tho' each of them have its respective outlet, yet they correspond to 12 little orifices about 2 lines broad, and ranged in 4 columns, which open into the stomach. When the animal is dead, these vesicles are found full of a white inodorous matter, of the consistence of pap, but there is a deal of probability that it is fluid while the animal is alive; this matter is doubtless the dissolvent of the food, which in the cold countries, and during the winter time, is only wood of the alder, elm, lime, ash, and some kinds of poplar; during the summer they eat all kinds of herbs, fruits, and roots, especially those of diverse kinds of water-lillies.

The intestines of this animal are very slender, and about 20 feet long. The *cæcum* is in figure of a scythe, being retained in that position by 2 ligaments which run, the one along its concave, and the other along its convex side, it is 18 inches long measured on the former side, and 30 on the latter. Its breadth is 4 inches at its large end, and may contain 5 or 6 lb. of water; the *colon* is 4 feet long, and the *rectum* about 15 inches.

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The bladder is like that of dogs ; and if the opening of the animal be continued to the root of the tail, we easily discern its testicles, and the bundle mentioned above in the description of the cutaneous muscle. This bundle is a hollow muscle which incloses the yard and testicles.

The testicles are situate in the groin, resting by their base on the lateral parts of the *os pubis*, and engaged in the fat. They are inclosed in several membranes furnished them by the *peritoneum* and muscles of the *abdomen*, especially by the *cremaster* muscles, whose fibres being circular, give them the figure of a cone : they are in all respects like those of dogs, when they are unravelled.

The *vasa deferentia* grow considerably bigger behind the neck of the bladder ; but they lessen again ere they enter the *urethra*, where they have outlets separate from each other.

The *vesiculæ seminales* are so engaged under the *os pubis*, that there is no seeing them without separating them. They are usually two inches long and one broad toward the middle, but pointed at the two ends : their passages also open separately into the *urethra*, and terminate like those of the *vasa deferentia*, in a fleshy eminence, about the size of a pea, being a species of *verumontanum* ; aside of this eminence we find several little orifices of the excretory ducts of certain glands, situate around the neck of the bladder, which do the office of prostrates, and are full of a white oily liquor.

The hollow muscle is situate between the *os pubis* and the aperture, at which the excrements are voided ; resembling, in some measure, an ancient kind of pouch, wide and round at bottom, and narrow towards the top. A tendinous body, an inch broad, ties it fast to the lower and middle

lip of the *os pubis*, from whence it descends continually winding to the common orifice above-mentioned.

Upon opening this kind of pouch, from top to bottom, about the middle thereof we discover the yard from its root to the *glans*. It divides this cavity into two, after which the hollow muscle, folding itself after a certain manner, forms two other cavities, situate under the first aside of the *glans*. In these four cavities are lodged the bags which contain the *castoreum*; tho', before we proceed farther, it may be necessary to speak of the common orifice. This is a cavity, about 2 inches every way, when well dilated, in which terminate the bags of the *castoreum*, *urethra*, *anus*, and the *vagina* in females; it is about 3 inches distant from the root of the tail, and 4 from the *os pubis*, being blackish, and bordered with a very fine hair, nothing like that of the rest of the body.

The root of the yard is fastened to the lower lip of the *os pubis*: hence it pierces the membrane of the *anus*, in the part where the upper bags communicate. This membrane adheres circularly to the insertion of the *glans*, as the *diaphragm* is to the *æfophagus*. The lower part of the yard, which is about 2 inches and a half long, is contained in the upper cavity of the hollow muscle, being the part where it separates into two; so that the *glans*, which is near an inch and half long, is intirely in the *anus*, between the outlets of the upper and lower bags. The castor attacks the female on the foreside, both on account of the situation of the common orifice, and by reason of the length and inflexibility of its tail.

The *glans*, which is perfectly like that of dogs, is covered with a skin grained like shagreen; in
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the body of the yard we find a pyramidal bone, whose base is fastened to the *corpus cavernosum*, and is about 15 lines long.

Under the root of the yard are two bodies, about the size of wallnuts, fastened to the *corpus cavernosum*. These two bodies consist of slender *vesiculæ*, which swell in the time of copulation, by means of several blood vessels which form a kind of *capsula* to the *urethra*.

In the same place, we find two oval glands, about 10 lines long, and 3 or four thick; their excretory vessels, which are about the thickness of a common bodkin, and upwards of an inch long, open into the *urethra*; about an inch deep in the yard, the substance of these glands are very firm, and contains a brownish oily liquor, which perhaps may serve to defend the cavity of the *urethra* from the acrimony of the urine. The like are found in rats, excepting that they are round.

The genital parts of the female castor are like those of the female hare, coney, or rat. The *vagina* of that of the castor is 5 inches long, and, like the *urethra*, is not inclosed in the upper cavity of the hollow muscle as the yard of the male is, but has its opening in the *anus*.

'Tis affirmed, that the females go 4 months with young, and bring 5, 6, or 8 at a time; yet M. *Sarrafin* has never found more than 4 in any of those opened by him.

The female castor has 4 breasts, two situate on the great pectoral muscle, like those of women, between the second and third of the true ribs, and two more on the neck, about four fingers higher than the former.

The ancients, who did not dissect with much care, could not perceive the castor's testicles, as being very small and situate in the groin. The size,

size, figure, and situation of the bags imposed upon them, and the first full discovery of these parts is owing to the royal academy.

The bags, or purses, contained in the upper cavities of the hollow muscle, and which we shall hereafter call upper bags, contain a resinous matter; but those in the lower cavities, which, for that reason, we shall call lower bags, are gathered in clusters, inclosed under a common membrane, and full of an oily matter. The upper are double, and resemble a wallet; each bag whereof being about 3 inches deep, and one and half wide at bottom, is placed, one on the right and the other on the left side of the yard. The bags describe a kind of semi-circle, as they approach the yard, and contract by little and little to the very apertures, which are about an inch in diameter, and look into the *cloaca*.

There are 3 membranes observable in the texture of these purses: the first simple, but very strong; the second much thicker, and beset with numerous vessels; the third is peculiar to the castor, being as dry as parchment, and about the same thickness, and tearing like it; but so folded on itself, that when opened, it extends to three times the space as before. It is very sleek on the outside, of a pearl colour, and frequently speckled with brown, sometimes with red: it is uneven on the inside, being lined with little threads, to which the resinous matter strongly adheres.

The first membrane seems only intended to contain the purses within their just magnitude: the vessels which beset the second supply the resinous matter which is mixed with the blood. This membrane follows all the folds of the third, as the pia mater enters all the sinusses of the brain; for the third, there would be a great deal of probability

bability of its serving to filtre the resinous matter, if any glands could be discovered in it; we are to suppose them however very small, and perhaps the threads above-mentioned may be their excretory ducts.

This matter thus filtrated thickens gradually in the purses, till it arrive at the consistence of a resin heated between the fingers, when it is called *castoreum*; it preserves its softness above a month, after its being taken from the animal, and smells very rank all this time, being brownish on the outside, and yellowish within; but it afterwards looses its smell, and turns hard and friable, like other resins. It may be added, that it is combustible at all times; the largest purses weigh about two ounces.

The lower purses, at first sight, seem double; one on the right, and the other on the left side of the *cloaca*; but upon taking off the membrane which covers them, we sometimes find two or three together, each cluster of these purses is 2 inches $\frac{1}{2}$ long, and 14 or 15 lines in diameter. The purses are round at bottom, and lessen insensibly as they approach the *cloaca*. The largest possesses the whole length of the cluster, but is only 8 or 10 lines in diameter. The second is not usually half so big as the first; and the third is commonly the smallest.

These purses, beside their common membrane, have each of them three proper ones: the first, being of a very delicate texture, is interspersed with abundance of vesicles. The second is not only thicker, but is likewise invested, and, as it were, crufted over with glands, which seem to be conglomerate, and are spread in clusters of different magnitudes over the whole external surface of the membrane. In the middle of these clus-

ters, we find certain cavities, which open into each other, *viz.* the larger into the smaller, and those at length into the purse itself by apertures, a line or two in diameter.

The third membrane is white, and so very delicate, that it tears as if it were only a cream condensed on the inner surface of the second. It is perforated in the same places as that other, in order to give passage to the liquor filtrated in the glands.

The first membrane sustains the blood vessels, which furnish the liquor to be secreted by the second and third. This secretion is performed by the glands, upon pricking them very slightly, yield an oily liquor, and even that in the purse readily empties itself at such punctures, upon slightly squeezing the purse. This liquor is of a pale yellow colour, full of little bodies, like those observable in oil of olives when it begins to fix; that of the castor afterwards becomes perfectly liquid, and of an amber colour.

The industry of nature is here truly wonderful, who, to prevent the little passages of the purses into the *cloaca* from being stopped, by the thickening of the liquor, or from shrivelling by the action of the air, has beset them all with a kind of hair about half an inch long, which arising from the purse itself a little beyond the passage, runs the whole length thereof, and advances a little into the *cloaca*.

All these purses, upper and lower, do not communicate with each other, their ducts, as already observed, terminate in the *cloaca*; nor do we know what use these liquors are of to the castor. There is no truth in what is commonly alledged, that they serve to excite its appetite when languishing; and M. *Sarrafin*, tho' he kept one
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of these animals 2 years, yet could not penetrate into their use. It is a mistake likewise, that the hunters make use thereof, as a bait for catching castors in snares; but 'tis customary to anoint the snares laid for predacious animals therewith, as foxes, bears, martins, but especially carcajoux, which last, in the winter time, attack the castors in their dens, which they frequently break up.

Among the savages the women grease their hair with the oil of the castor's bags, but it smells ill, and can have no charms, except for a savage.

From the *abdomen* we must pass to the *thorax* of the castor; this part is about 5 inches long, very narrow at top, much wider towards the bottom, and close by 14 ribs, *viz.* seven true ones, which are very short, and seven spurious ones, which are not only much broader, but leave a considerable space between them. 'Tis by this means the castor is enabled to contract itself so easily, since the ribs are quickly drawn together by the action of the circular fibres of the first muscle.

The *sternum* consists of 5 narrow bones; the scutiform cartilage, which is about an inch broad, is round and very flexible; the lungs have 6 lobes, 3 on the right side, two on the left, and a small one inclosed in the *mediastinum*; the annular cartilages of the *trachea* consist only of one piece.

The heart is about 2 inches long, and the base thereof somewhat above an inch and $\frac{1}{2}$ in diameter; its ventricles are pretty equal, but the right auricle is much smaller than the left, and yet I do not hereupon conclude, that the quantity of blood, falling into this ventricle, is less proportionably to its bulk, than what is received by the other; for the lower *vena cava* is here considerably widened, and forms a kind of bag,

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surrounded with fleshy fibres, being about an inch and $\frac{1}{2}$ in diameter; this bag acts in concert with the right auricle for filling the right ventricle, and being narrower next the liver, is closed by 3 valves like the *sigmoides*, which permit the blood to pursue its course, but hinder its reflux, which otherwise there would be danger of, by reason the upper *vena cava*, in lieu of opening into the auricle, goes backwards, and empties itself into this bag, so that the two streams of blood here meet in opposite directions; and the left subclavian, in lieu of pursuing its course into the upper *vena cava*, descends over the lower branch of the *aorta* under the base of the heart, and opens into the *sacculus* abovementioned.

As to the head, the most remarkable things therein are, 1st, the occipital bone, which is placed like a plate on the back of the head; 2dly, there is no inner sinus in the *falx* of the *dura-mater*, which membrane makes a slight division of the brain, being sustained by ossicles, inserted in its own substance, some whereof are only bony *laminae*, very solid tho' thin, while others are round, about a line in diameter, and 2 or 3 lines long; 3dly, the brain has no sensible sinus's, but the *pia mater* separates from it, as if it were only laid on a smooth body; 4thly, the *cerebellum* has several bunches or prominences of different figures, which are separated from each other by the *pia mater*; two of these spring from the sides, and are about $\frac{1}{3}$ of an inch every way; 5thly, the eyes are very small, the aperture of the eyelids not being above a third of an inch; the *cornea* is round, and the *iris* of a deep blue; 6thly, there is a third sort of eye-brow, situate in the large *cantbus* of the eye, which serves as a curtain to cover or uncover the *cornea*, at the dis-

discretion of the animal; 7thly, the two jaws, which are very strong and nearly equal, are each furnished with 10 teeth, 2 cutters, and 8 grinders; the cutters are situate at the end of the snout; those on the upper side are about 8 lines long, and those on the under side a whole inch; the roots or twangs of the former are 2 inches $\frac{1}{2}$ long, and those of the latter three, following with all the curvity of the jaws, which gives them an incredible strength; accordingly the castor will fell the largest trees by meer dint of teeth; 8thly, as these animals live chiefly on dry foods, nature has furnished them with salival glands of prodigious bulk, to furnish moistening enough; they possess all the under part of the lower jaw, the fore-part of the neck, and descend as low as the clavicles; they are covered with a muscle, which adheres to the skin, and consists of 2 plans of fleshy fibres, fastened to the second, third, and fourth *vertebrae* of the neck, by a fleshy origin 4 fingers broad; these 2 plans, taking contrary courses, invest the neck towards the *trachea*, whereon their fibres cross each other, somewhat in manner of a mat; that arising from the right side, proceeds towards the left, and is inserted by its *aponeurosis* into the arm, the bend of the elbow, and the first joint. The other plan, taking an opposite course, is inserted after the same manner into the other arm; this muscle is fastened a-top to the whole lower jaw, and at bottom is sustained on the fat, and descends to the clavicles; its use is to squeeze the glands, by forming the jaw, and bringing the arms of the animal nearer, at the same time that it holds its food in its hands.

The castor's tail has no relation to the rest of its body; it seems rather to approach the nature

of a fish, being covered over with a scaly skin, under which we find a firm kind of fat, much like the flesh of a porpoise, which was probably what chiefly contributed to the castor's passing for an amphibious animal. The scales are hexagonal, $\frac{1}{2}$ a line thick, and 3 or 4 long, being joined together by a thin pellicle, and set in the skin, from which they easily separate, after the animal is dead. Between each scale arise three or four hairs, about 2 lines long, which are more numerous on the sides of the tail than elsewhere.

For the motion of this tail there is a great number of muscles, some large and others smaller; the largest are fastened on the transverse *apophyses* of the *os sacrum*, and their tendons distributed in bundles of 4 or 6, each enclosed in sheaths, which carry them along the *vertebrae* of the tail. The tendons of the lesser muscles are fastened and confounded along with those of the first.

The castor being destined for works of architecture, fells and cuts wood with its teeth, softens and kneads the clay with its feet, and its tail not only serves for a trowel, but as a hod for carrying mortar; and hence the reason of its being scaly furnished with fat, and with a multitude of muscles.

The fore-feet are like those of other animals, which hold their meat between their feet, and thus bite it by degrees, as rats, squirrels, &c. but the hind feet have no relation thereto, being liker those of river fowls, and which are webbed-footed, as ducks, geese, &c. so that the castor is fit either to walk on the ground, or swim in the water. From the tip of its nose to the thighs, it resembles a rat; but from the thighs to the tail, it is more like one of the flat-footed kind of river toad.

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To the anatomy of the castor, M. *Sarrafin* has added several observations relating to their manner of living.

First, when the large floods are over, the females return to their cells to bring forth their young; the males keep the field till *June* and *July*, nor come home till the waters be quite low; they then betake themselves to repair the damages which the floods have made in their houses, or else make new ones; they change their place chiefly on three accounts, 1st, when they have spent all the food within their reach; 2dly, when the company is too numerous, and, 3dly, when the hunters molest them too much.

Secondly, To fix their dwelling, they chuse a place fruitful in provisions, watered with a little river, and fit for making a lake therein; they begin with building a causeway of a sufficient height, for raising the water to the first bed or apartment of their cells. If the country be flat and the river deep, the causeways are built long, but not so high as in vallies; these causeways are 10 or 12 feet thick at their foundation, but diminish as they rise, and a-top are rarely above two. The animal having a great talent at felling wood, does not spare it, but cuts it usually in pieces about the thickness of the arm or thigh, and from two feet to six feet long; these they use as stakes, sinking one end of them deep in the ground, and very close to each other, waiting them with other smaller and more pliable pieces, and at length filling up the vacuities with clay. This work they continue while the water rises for the greater conveyency of transporting their materials, but at length they put a stop to this kind of dyke; when the waters retained may reach the first
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stratum of the habitation intended. That side of the caufway, contiguous to the water, is built ftopping, fo that the water acting in the direction of its depth, preffes it ftrongly againft the ground; at the fame time the oppofite fide is perfectly perpendicular; this work is ftrong enough to bear people who get upon it, and the animals take great care to keep it in order, never failing to repair the fmalleft breach with clay; if they perceive that the hunters obferve them, they only work in the night-time, or elfe quite abandon their dwelling.

Thirdly, when the caufway is finifhed, they go to work on their cabbins, which they always build very ftrongly at the edge of the water, on fome little ifland, or in piles drove for the purpofe. Thefe lodges are either round or oval, and ftand about $\frac{2}{3}$ out of the water, but they take care to leave a door, which the ice cannot ftop; fometimes they build the whole houfe on dry ground, and dig ditches five or fix feet deep, to the edge of the water. The fame materials are ufed for their houfes as the caufways, tho' the former are perpendicular, and terminate in a kind of dome, the walls being ufually two feet thick; as their teeth are equivalent to the beft faws, they cut off all the ends of timber which come beyond the walls, and plaifter them over both infide and out, with a kind of mud made of clay, and dried herbs, and here their tail ferves them in good ftead to beat and lay this plaifter the clofer.

Fourthly, the infide of the cell is roofed with a flat vault, and may ferve to lodge 8 or 10 caftors; on the out-fide it may meafure 8 or 10 feet broad, and 10 or 12 feet long, fupposing it oval; within-fide it may be 4 or 5 feet

feet broad, and 5 or 6 long, if the number of castors to inhabit it be 15, 20, or even 30, which however rarely happens. The house is made large in proportion, or even several houses are built close to each other. Some missionaries assured M. Sarrafin, that they had found four hundred castors lodged in different cells, which communicated with each other; they are built in stories, that the animal may have a retreat when the waters swell; they have also an aperture beside the door, and the place where they bathe; by this aperture they go to the water, to void their excrements.

Fifthly, the name terrier castors is given to such as lodge in caves, built where the ground rises over the edge of the water; these begin their house by an aperture, which goes farther or less into the water, as the ice may be more or less thick, and continue it 5 or 6 feet long, but make it no broader than is necessary to afford them passage; this done, they make a lake 3 or 4 feet every way, wherein they bathe themselves at pleasure; then they dig another gut in the ground, rising in stories one over another, that they may always place themselves dry when the waters swell; these guts are sometimes a hundred feet long; the castor covers the place where it lies with grass, and in winter makes a kind of shavings, which serves it for a quilt.

Sixthly, All these preparations, especially those of the castors, which inhabit cold countries, are usually finished in *August* and *September*, which is the time when they are to begin making provision for the winter's subsistence; they cut wood therefore in pieces, from 2 or 3 feet long, to 8 or 10, the largest whereof are drawn by several of these animals together, and the smaller
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by a single one, but all in different roads, to prevent their obstructing one another. They first lay a certain quantity of these to float in the water, then lay new ones on the first, and continue thus heaping one piece over another, till their store be answerable to the number of animals which intend to live together. Thus the provision for 8 or 10 castors is 25 or 30 feet square, over 8 or 10 feet deep. This wood is not laid like that in our yards, but is so disposed, that they may pull out what piece they please, for they only eat those which dip in the water; before they eat them they chop them small, and bring them into that part of the cell where they lie; if they had chopped ere they laid them up in their wood-yard, the water would have dispersed and driven them asunder.

Seventhly, As to the castor-hunting, it is performed from the beginning of *November* to *March* and *April*, they being then well furnished with hair. The way of taking them, is either by lying close and shooting them, or laying snares, or making holes for them in the ice: the first is the most tedious, as well as the most precarious way; the second is the most common, tho' the castors have sufficient provisions, yet they will be going from time to time into the woods to seek for new food, and the hunters, who know that they love fresh wood better than that which has been long kept, bring it them near their houses, and lay traps for them, like those wherewith we catch rats, several stakes 3 or 4 feet long, being driven deep into the ground, there is one heavy cross stake raised a foot and a half high, under which, by way of bait, is placed a branch of poplar, 5 or 6 feet long, which leads to another very slender branch. This last
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corresponds so nicely to the cross piece, that the castor may shake the first as long as it pleases, the cross piece never falls till it begins to know the little branch, and then it is infallibly a prisoner.

Eightly, The last method is to dig holes in the ice, when not above a foot thick, the castors will not fail coming to these holes to breath, and here the hunters are ready to attack them with axes. Some hunters will fill up these holes with flocks of typha, to prevent their being spied by the animals, and in this case catch them by a hind foot. If there be any rivulet near the cabbins, they cut the ice a-cross, and lay a strong net therein, which done, going to storm the cabbins and drive out its inhabitants, they naturally run for safety into the rivulet, and throw themselves into the snare.

VII. *The description and use of a level of a new construction, by M. de la Hire **.

The great aqueducts, which the ancients have made, might have persuaded us, that they were very skilful in the art of levelling, if the instruments which they used, and the whole artifice, which they employed, had not been handed down to us in the works of *Vitruvius*. Their great level, which they called *chorobates*, was a piece of wood 20 feet long, supported by some pieces at the extremities, and which had in its upper part a canal that they filled with water, with some little plummets, which hung to the sides, to be assured if the piece was in a level; and this was the whole extent of their levelling, for they carried the *chorobates* from 20 feet, to 20 feet to conduct their work.

* Nov. 12, 1704.

But the new discoveries that have been made in the academy of sciences, and the different levels that have been there invented with telescopes, which serve them for sights, have given us the means of making levels with a great deal more justness, and with much more ease, than all those which have been made till this time, since we may level at once a distance of 1000 toises, without any sensible error.

We have in our hands the levels of M. *Picard*, *Mariotte*, *Huygens*, *Thevenot*, and *Roemer*, whose constructions are all different, and I have also given one which draws its justness from the surface of water; a description of which I have printed in M. *Picard*'s treatise of levelling in 1684, and which was afterwards copied in the memoirs of the academy in 1699. But the surveys that I have formerly made by order of the king, in different places and at different times, for the space of about 100 leagues, have shewn me, that almost all these levels have great inconveniences in them, and that they cannot easily be transported without our being obliged to rectify them, and sometimes to restore the sights, which require pretty troublesome operations. Not that it is necessary to make use of a just level to make exact surveys, since we may easily arrive at it by taking some precautions, as is observed in M. *Picard*'s levelling, and as I have explained it afterwards in a little treatise of levelling, which I have given to the publick.

The * level, which I here describe, and which I made sometime ago, is constructed upon a different principle from all other levels which have hitherto appeared: for they draw all their justness, either from the centre of gravity of the body of the whole level, which is suspended upon

a body flexible or otherwise, or from a plummet also suspended to a thread, as fine as a hair; or lastly, from the surface of water, or some other fluid body, whose surface is always level. But this here is not at all suspended by any body whatever; on the contrary, the centre of gravity of the body which serves it for a rule, as it does to many others, is placed above the point of support; so that if it was possible, that the centre of gravity of the body, which is a mathematical point, should remain immoveable upon the support, which is the nicest point that it is possible to make; we should then have the true level, which would be the perpendicular line drawn to that which passes through the centre of gravity and through the support. But as it is impossible to make the centre of gravity stay in a fixed place above the point of support, we take for the true level the position where this point is indifferent to fall either on one side or the other.

It will perhaps be said, that this principle is not so just as that, where the heavy body is suspended by a flexible body; but if we make the least reflection upon these suspensions, we shall see that there are very great irregularities caused by the nature of these bodies; besides, if the whole machine is exposed to a little wind, it is in a continual shaking and agitation, and it is impossible to determine the level. I say nothing of the remedies, that are brought to these shakings, as of plunging the weight suspended to the machine in water or oil to stop the vibrations, since even this remedy has great inconveniences, and yet is not sufficient.

This which I propose is firm, solid and steady, being once well constructed, and it may be carried about as we please, without being afraid

that it may alter; and if it makes any alteration, it will be very easy to rectify it without changing its place, or, as we say, a single station.

The new sights upon glass, as I have proposed them to the academy, and as they are described in the memoirs of 1700, and in my astronomical tables, give it also a very great advantage above all the rest; for these sights are unalterable by all the changes of the air, and cannot be spoiled by any other accident whatever, tho' they should be at least as fine and slender, as the threads of a silk-worm, which have been hitherto considered as the most fit to make exact observations with. It is very easy to make them, since it is only a little line, or very fine stroke, marked with a diamond upon a little bit of glass, which is fixed to the *focus* of the object-glass of the telescope; for the centre of the object-glass is in the room of an object-sight, and the stroke upon the glass serves for an eye-sight. It is easy to judge, that this eye-sight cannot suffer any alteration or change, either by heat or cold, by little insects which fix themselves to the silk, by touching it, or, in fine, by carrying the instrument in any manner whatever; for it will be as solid as the object-sight.

The principal part of this level is an iron rule, which bears at its extremities the sights or diopeters, which are an object-glass of a telescope for the object-sight; and for the eye-sight, a little bit of glass with a little stroke, as I have just said. This glass runs in a frame or groove, that it may be stopped at what height we please. This rule is instead of the body of a telescope, which has no eye-glass, and has no other tube than the whole box, which is blacked on the inside and well closed.

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There is under this iron rule another rule placed upon the ground, which serves to make it more firm, and has in the middle another piece of iron or brass, about $\frac{2}{3}$ of the length of the first, with which it is placed in a square. This piece increases in width at the bottom, so that the breadth is perpendicular to the length of the rule of the telescope. At the extremity of this piece are soldered on both sides the pivots, upon which the machine moves; and consequently as the whole body of the level is moved when the pivots are pretty near upon a level, the telescope rises and falls in its motion.

There is a little care required in the construction of the pivots. Their cut must be in form of a very acute lozenge, with a sharp point, both at top and bottom; for it is by these places that the whole level is sustained upon the support.

The pivots are of tempered steel, and the place where they bear upon the support is a little hollow, altho' sharp, that in the motion of the level, they may not get from either side. There are two similar cavities opposite to one another for an use which we shall hereafter explain.

The supports are fastened with screws to a particular piece, which is of wood, and this piece is held in the bottom of the box with screws, in such a manner, that it may be drawn out of the box with the whole level which rests upon its supports:

These supports are made of a flat piece of tempered steel, in which are two round apertures, of which the inner one is blunted, to sustain the pivots of the level, and to be firm enough not to spoil, in the motion of the level.

The box that incloses the level, may be made of what figure you please on the outside; but the

the whole level being of the figure of a T, the box ought to be pretty near the same figure, and it must hold the instruments in such a manner, that it has only liberty to move a little on each side. At the two extremities of the upper part of the box, which is the transverse of the T, there are two round covers, to one of which is fixed the end of the tube, where the *porte oculaire* enters; for the eye-glass is not fastened to the telescope, and we may by this means bring it nigher or farther from the eye-sight, according to the strength of the observer's sight. The aperture of the other end of the box serves to let the rays of the objects pass, which meet the object-glass, or object-sight.

Toward the eye-glass at the lower part of the length of the box, is a pretty long screw, which moves in a nut fixed to the box. This screw serves to raise the end of the iron rule, as occasion requires. The furrows of this screw are very fine to raise the rule as it were insensibly by turning the screw.

Opposite to the screw, that is, in the top of the cross-way of the box, there is within a very slender spring, which is held to the box by the extremity farthest from the eye-glass, and at the other it bears a peg with a button, which passes to the out-side of the box. This peg serves to drive the spring downwards; for in its natural state it is applied against the top of the box on the inside.

There is at the screw a *repaire*, or mark, to let it into the nut just in this place, and then the rule of the telescope, being rested upon the screws, the telescope is over-against the apertures of the box.

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There is then nothing to be done but to place the sights, so that their true line be perpendicular to the plane, which passes through the centre of gravity of the whole level, and through the line of the supports, and this is called rectifying it; and as all the parts that compose it are solids, being once well rectified, it will not afterwards change: wherefore it is sufficient to rectify it well in constructing it, by some of the ways which are explained in M. *Picard's* treatise of levelling, by establishing two points of level, and also with the level without being rectified.

To rectify the level.

Supposing then that we have two points of level A and B, about 100 toises distant from one another, we place the telescope at one end of these points as A, and point it toward the other point B, raising or sinking the box gently. And when the object B appears upon the eye-sight, if the rule of the telescope is placed upon the point of the screw, and it is not in a state of falling forward, that is, toward the object-glass, which we may know by pushing or raising the rule a little, by turning the screw; for then the telescope falls lower, and it is also placed upon the screw. This shews, that the part of the whole level, which is towards the eye-glass, is too heavy; therefore the part of the rule toward the object-glass must be changed, by means of a little weight which shall run upon the rule, and may be stopped in whatsoever place we please with a little screw, or only charging it with a little soft wax and lead. We charge this part of the rule so much, that the sights being pointed toward the object B, are indifferent to rest upon the point of the screw, or to fall forward, and then it will be rectified.

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But if at first the sights being pointed toward the object B, the rule cannot keep upon the screw; we shall know by that the part of the level toward the eye-glass is too light, and then it must be charged as was done in the other case, till the level is in an indifferent state of falling or resting upon the point of the screw; and then the level will be rectified.

We might also, instead of charging the rule on either side, raise or sink the eye-sight in its frame, till the level was just; which we might do if the level is not far from being just.

We shall observe, that in these operations every time that the telescope falls forwards, it is replaced upon the point of the screw, by pushing the spring at the top of the box, which drives the frame of the eye-sight downwards, and by that means replace the rule upon the point of the screw.

As this level may be put in a reverse situation, which is when the telescope is below the pivots, we must explain what ought to be observed in the constructing of it to serve in this state.

It is certain, that if the pivots were a mathematical line, when the level is reversed, it would point to the same place where it pointed in its right situation, provided that this place was perfectly level with the place where the level is situated; and to rectify it, we need only aim the telescope at the same point in the two situations of the level, by increasing or diminishing by degrees the weight of one of the ends of the rule. But as the pivots must have a considerable thickness to make them solid, it may happen that the plane, which passes thro' the centre of gravity of the whole level, and through the line where the pivots rest, will not be the same in the two situations

situations of the level ; it must be rectified in the reversed position, the same as in the right. But as we must not change the sights which are rectified, for the right position, we must correct the place of the pivots, where they bear upon the support in the reverse position.

The level being therefore reversed, if the sights do not give the same point, as in the right situation rectified, the pivots must be filed a little to drive the point toward the object glass, if the sights fall too low in the reverse situation, or toward the eye-glass if they rise too high.

The pivots may also be made in another manner, so that the plane which shall pass through the centre of gravity of the instrument, and through the place where the pivots rest, shall be the same both in the right and reversed situation of the level, and thus we may rectify this level from a single station, without having a line of level.

In this second manner, the pivots must be cylindrical, and very well turned and polished, in the places where they bear upon and touch their supports, which ought to be a little hollow like a pulley. Besides, the bottom and top of the hole of the support which sustains the pivots, must be almost in a right line, that the pivots may not touch sensibly but in one point.

It is evident, that in this construction of the pivots, the centre of gravity of the level, and the line which shall pass thro' the places where the pivots rest in the two situations of the level, will be always in the same plane, which shall also pass thro' the axis of the cylinder of the pivots.

This suspension of the level is not so nice as the first, because the place of the pivot, where it touches upon the support, is of a circular fi-

gure, and in the first it is of a pointed figure a little blunted.

The use of the level.

To make use of this level, we must first put the screw to its mark, that the sights may be over against the apertures of the box. Afterwards, by the means of the springs, we drive the rule of the telescope against the end of the screw, in such a manner, that it does not fall from the other side, which is easy to do by inclining the box; and in this state, the rule of the telescope being placed upon the screw, without the spring holding it, we bend the box by degrees toward the end, where the object glass is, till the telescope falls on that side. Then we hold the box as firm as possible in this state, either upon a foot or against some other solid body; the telescope must be then pointed toward the object, that we would level, and we ought to observe exactly the part of the object that appears upon the stroke, which serves for an eye-sight. But as I suppose the telescope to be placed upon the screw, we must push the screw gently, till the telescope falls on the other side, and observe well the part of the object, which appears upon the stroke, when the telescope falls; and to be assured of it we must drive back the telescope upon the screw by means of the spring, and observe if it falls again; and if the same object appears upon the stroke of the glass, which appeared there before. We should then undo a very little part of the screw, and driving back the telescope upon the screw, observe what difference there shall be between the object, which shall appear upon the stroke in this state of the telescope, and that which appeared there before. It must also be

considered, if the telescope rests then upon the screw, or if it falls again from the other side; for to have the point of the level just, the telescope being placed upon the end of the screw, must be in a state of falling on the other side, without falling, and we cannot raise it the least with the screw without its falling; it is then that the object, which appears in the telescope upon the stroke of the sight, is in a level with the stroke of this sight.

What I say of the level, must always be understood of the apparent level, with relation to the place where the instrument is, which serves to level, as it is explained in the treatise of levelling; for to have the true level corresponding to that where the instrument is placed, we must make the correction to the apparent point of level, with relation to the distance between the place where the instrument is, and the point levelled; but we do not here treat of the practice of levelling.

One of the advantages of this level is, that it may be reversed, and serve again to level in this situation, without any preparation or change; and tho' it has then some vibrations, they last but a little while; for they come only from the body of the instrument, and not from the motion of the box, which must be held firm, but without any subjection, since it loses nothing of the justness of the level, we may also easily stop the shaking of the level, by supporting gently the spring against the frame of the sight, and by letting it afterwards replace in its natural state.

The size of this level is not at all determined; for the greater the telescopes are, the more distinct the distant objects will appear; but it will be much more inconvenient to be carried about.

I have proved that one of these levels, whose height and length was but 10 inches, determined the level to near 3 or 4 inches at a distance of 1000 toises, which is all we can hope from a level, since an object of this bigness, at this distance, is entirely covered by a single thread of silk.

We may make use of different supports; but one of the most convenient ways for practice, will be to fix the box of the level very firm with a screw, against the cross of a painter's easel, which is solid; we might also hold it only in the hand, and bear it against some very stable body, whilst the operations are performed.

It must be observed, that when we would transport this level, the pivots should not bear at all upon their support, which will be very easy, if these pivots jut a little out of the box, and if there is in this place upon the box two sorts of hooks, which are engaged in the ends of the pivots, and hold all the level raised out of the supports. We might also fix the telescope against the end of the screw, by means of the spring, which shall be rested upon the sight; but if we would open the lid of the box, we might engage the whole level between some brackets, which will hinder it from balancing on one side or other, in carrying it a long journey.

Pl. IV. Vol. II.



Figure of
the Level
proposed
in its box
with the fore
part removed

J. Mynde sc

T A B L E

OF THE

PAPERS CONTAINED IN THE ABRIDGMENT
OF THE HISTORY AND MEMOIRS OF
ROYAL ACADEMY OF SCIENCES
PARIS, FOR THE YEAR MDCCLXX.

IN THE HISTORY

- I. Of the different kinds of the Sun
at different times.
- II. Of a great number of which bodies are to
be expected in a burning matter.
- III. Of some extraordinary lights seen in the
night.
- IV. Of an unusual fire in the town of Angou-
leme.
- V. Of some extraordinary lights seen at Angou-
leme.
- VI. Of a sort of sea-breeze found in France.
- VII. Of the rising of the wind, which follows
the division in the division of the
air.
- VIII. Of the preservation of water in the
vacuum.
- IX. On a method of preserving inland waters
the water.
- X. Of an experiment that was made at Montpelier.
- XI. Of the kind of the burning matter in the
vacuum.
- XII. Of the colour of the water.
- XIII. Of M. De L'Isle's map of the Roman Empire.

A
T A B L E

OF THE
PAPERS contained in the ABRIDGMENT
of the HISTORY and MEMOIRS of the
ROYAL ACADEMY of SCIENCES at
PARIS, for the Year MDCCCV.

In the HISTORY.

- I. **O**N the different heights of the Seine;
at different times.
- II. Of a proper vessel in which bodies are to be
exposed to a burning mirror.
- III. Of some extraordinary lights seen in the
heavens.
- IV. Of an unusual smell in the wines of Anjou.
- V. Of some extraneous fossils found at Anjou.
- VI. Of a sort of bee-hives found in stones in
the Upper Egypt.
- VII. Of the rolling of the drops, which fall from
the alembic, in the distillation of spirit of
wine.
- VIII. Of the preservation of water in long
voyages.
- IX. On a method of preserving inlaid work from
the worms.
- X. Of an excessive heat felt at Montpellier.
- XI. Of the effects of the burning mirror in that
weather.
- XII. Of the yellow amber.
- XIII. Of M. De L'Isle's map of the Roman empire.

In

In the MEMOIRS.

- I. *Observations on the quantity of rain water, which fell at the royal observatory during the year 1704, with the heights of the barometer and thermometer, and observations on the winds that reigned, by M. de la Hire.*
- II. *A comparison of the observations on the rain and winds made by M. de Pont-briant, at the castle of Pont-briant, two leagues from St. Malo, and towards the sea-coast, during the year 1704; with those which were made at the observatory at the same time; by M. de la Hire.*
- III. *Reflections on the observations of the variation of the needle, made in the pope's legat's voyage to China, in the year 1703; by M. Cassini the son.*
- IV. *Of barometers without quicksilver, for the use of the sea, by M. Amontons.*
- V. *Reflections on the rules for the condensation of the air, by M. Cassini the son.*
- VI. *That the experiments used to prove that fluids are first condensed and cooled before they are dilated at the approach of heat, do not prove it, and that this apparent condensation is merely the effect of the dilatation of the glass and of the vessels which contain these fluids, by M. Amontons.*
- VII. *Observations on the declination of the needle, made in a voyage from France to the East-Indies, and in the return from the Indies to France, in the years 1703 and 1704, by M. Cassini the son.*
- VIII. *New observations on the load-stone and on the magnetical needles, by M. de la Hire the son.*

- IX. *On the condensation and dilatation of the air, by M. de la Hire the son.*
- X. *Experiments on the rarefaction of the air, by M. Amontons.*
- XI. *On the ecumes printanieres, or cuckow-spittle, by M. Poupert.*
- XII. *Philosophical experiments on the refraction of musket-balls in water, and on the resistance of this fluid, by M. Carré.*
- XIII. *A comparison between our observations on the barometer, and those made by F. Sebastien Truchet, by M. Maraldi.*
- XIV. *Remarks on some experiments made with several barometers, and on the light made by one of them, which was agitated vertically, by M. de la Hire the son.*
- XV. *On the height of the quicksilver in the barometers, by M. Amontons.*
- XVI. *A continuation of the same observations, by M. Amontons.*
- XVII. *A continuation of the same observations, by M. Amontons.*
- XVIII. *Experiments on the capillary tubes, by M. Carré.*
- XIX. *A continuation of the observations on the height of the quicksilver in the barometers, by M. Amontons.*
- XX. *New reflections on the rules for the condensation of the air, by M. Cassini the son.*
- XXI. *An experiment on the quantity of heat reflected to us by the moon, by M. de la Hire.*

AN
ABRIDGMENT
OF THE

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1705.

I. *On the different heights of the Seine at different times; translated by Mr. Chambers.*

EVERY thing is to be observed, and the present obscurity of physicks is not more owing to this, that causes are hid, than that effects themselves are yet undiscovered. M. *Amontons* had begun to procure observations of the different heights of the *Seine*, at different times, by means of a friend, whose house was commodiously situate for that end, and who had taken a fixed point on the massive of *Pont-Neuf*, which supports the equestrian statue of *Henry IV.* Hence he told day by day the risings or fallings of the *Seine*, upon an immovable scale fitted thereon, which he could observe with a telescope. M. *Amontons*, having the journal of these observations, from the 14th of *September*, 1703, to the last of *December*, 1704, digested them in the following manner.

He divided the whole into rises and falls of water, noting, 1st, for instance, how many days the water continued rising from the beginning of the observations, and how much it had rose; then how many days it continued falling, and what

what the whole fall was; then again, how many days it had risen, and so on.

By the bare journal of these observations it appeared, at what time of the year the water had been highest or lowest; how much it had been higher one year than another, &c. and by this division of the observations, into rises and falls, the number of rises and falls each year; with their duration, magnitude, and other relations, exactly appeared.

For an instance, from the 14th of *September*, 1703, to the 10th of *February*, 1704, there appeared to have been 8 rises, all which together made 223 inches, and it continued 77 days; that from the 10th of *February* 1704, to the 18th of *September* following, there had been 8 more elevations, which only made 163 inches, and it continued 70 days. Hence M. *Amontans* inferred, that the rains whereby the *Seine* is swelled, had been more hasty, and succeeded each other faster from the autumnal equinox of 1703, to the vernal equinox of 1704, than from this last equinox, to the autumnal one following; inasmuch, as the sum of the former elevations was almost double that of the latter, tho' in times nearly equal.

As to the different falls of the water in the same times, their quantity bore a nearer relation to their duration; hence it may be inferred, that as the waters do not fall so readily as they rise, 'tis probable that rivers while they are high, send waters into the ground, which return to them again, and serve to support them.

These notions we only give as a specimen, of the consequences that might be drawn from a sufficient number of just observations, upon the heights of rivers at different times; they who

have any relish for philosophy, and have opportunities of making them, we hope will be invited hereby to put their hands to the plow.

II. *Of a proper vessel in which bodies are to be exposed to a burning mirror.*

The bodies which are exposed to the burning mirror of the royal palace, can only be put into a great hollow coal, because all other vessels either melt or break with so great a fire. But M. *Homborg* has observed, that this coal must be of green wood, and not of dry. The latter is full of cracks, because when they make it, the flame passes thro' the wood with too much rapidity, and in too great a quantity, and consequently it is less proper to contain the bodies in fusion, which we would preserve.

III. *Of some extraordinary lights seen in the heavens.*

Father *Laval* a jesuit, who is at *Marseilles*; and Mess. *de Plantade* and *Clapiés*, who are at *Montpellier*, sent to M. *Cassini*, with several astronomical observations, the account of a luminous phenomenon, which had been seen Dec. 26, 1704, at 5^h 30', in the evening at *Marseilles*, and at 5^h $\frac{1}{4}$ at *Montpellier*. We cannot doubt by the circumstances of the two accounts, but that they were the same. At *Marseilles*, where it was best observed, F. *Laval* saw a very luminous beam driven pretty slowly from E. to W. the wind was E. It came from near *Venus*, at least as could be judged by the eye, and went to the sea where it plunged, at most two leagues broad. There were seen before at *Marseilles*, or thereabouts,

abouts, 2 beams like this, and having the same motion. At *Montpellier* they saw, at the hour mentioned, a globe of fire fall at some distance from the city. The air was then very clear and calm, and a faint yellow colour tinged all the W. for more than 10 degrees in height.

IV. *Of an unusual smell in the wines of Anjou.*

M. *Lemery* was told by M. *Delisle*, apothecary at *Angers*, that the best wines made at *Anjou* in 1704, had, in a fortnight or month after they were made, a smell of burnt horn, which only increased with time. It always retained it very much, altho' they changed the casks.

V. *Of some extraneous fossils found at Anjou.*

The same M. *Delisle* found at *Anjou*, in a shallow quarry, very distant from rivers and ponds, some of the pretended tongues of serpents petrified, such as are found at *Malta*, and which are in reality the teeth of sharks petrified.

He found also in a quarry, where the stone is very soft and hardens afterward in the air, an infinite number of little figures of shells, which in some places had only the first traces, and were a sort of embryos; in others they were more formed, and in others perfect.

We may add to these observations, what has been said upon the same subject, in the hist. of 1703 *.

* Page 13 of this volume.

VI. *Of a sort of bee-hives, found in stones in the Upper Egypt.*

M. *Dodart* having received from M. *Lippi*, who travelled into *Ethiopia* with M. *du Roule*, the king's envoy, a letter dated from *Siout*, in *Upper Egypt*, Sept. 5, 1704, which containing a singular fact, he communicated it to the company. M. *Lippi* found upon the mountains of *Siout*, at the entry of a vast cavern, a body of real stone of an irregular figure, quite porous, which he had the curiosity to open. He was very much surpris'd to see it all divided into oval cells of 3 lines broad, and 4 long, placed even every way, and not communicating at all together, lined on the inside with a very fine membrane, and, what is the most wonderful, each inclosing either a worm or a *chrysalis*, or a fly perfectly like a bee. The worms were very hard and solid, and might pass for petrified; the *chrysalides* and flies were not, but only dried and preserved like the ancient mummies. Many of the flies had under them little oval grains, which seemed to be eggs. There was at the bottom of many of the cells a thick juice, blackish, very hard, which appears red against the light, very sweet, making the spittle yellow, and inflaming it like a resin. It is in short real honey. M. *Lippi* imagines that it is a natural hive, which was at first formed of a loose, light, and sandy earth, and which afterwards, by some particular accident was petrified. The animals which inhabited it had been surpris'd by the petrification, and were fixed in the state in which they were then found. Their dried mucosity had formed the membrane which lined the cells. When the hive was

was soft, the worms and flies went out to search for food, and the flies made their honey in it.

Looking in the same place for new light into this fact, M. Lippi found in many places the beginnings of like hives. These were as the first bed, formed of a number of little cells, of which the most part were open, and contained the animal, either in a worm, a *obrysalis*, or a fly, but dry and very hard, as were also the hives that were begun. He saw also upon one of these first beds, a second composed of a heap of little bosses, about 5 lines high, and an inch in diameter at the base. They were crumbling, easy to reduce into powder, and very like little mole hills. M. Lippi opened them by striking them very lightly, and he always found 2 or 3 oval cells quite filled with yellow worms, which were full of juice.

It is easy to imagine, that upon the first bed once formed, there were others formed afterwards, which composed the whole hive; but how are these beds formed? From whence comes the earth of which they are made? Did the animal bring it? And how did it bring it, and in so great a quantity? We know no more yet. Time can only bring this knowledge.

VII. *Of the rolling of the drops, which fall from the alembic, in the distillation of spirit of wine.*

M. Homberg says, that in the distillation of spirit of wine, the drops which fall from about a foot and half high from the spout of the alembic on the liquor already distilled, roll thereon like pease on a table, that from the greater height

height they fall the better they roll. So that were they to fall only one inch, this would not happen, and also they roll so much the better the hotter they are, and that were it water instead of spirit of wine, the experiment would never succeed.

He says, that all the parts of sulphurous liquors being impregnated with the matter of light, and their whole surface being set round with it, and this so much the more the hotter they are, or that by the greater length of their fall, they have collected a greater quantity of it in the air, this matter performing the same effect as an infinite number of little points would do proceeding outwardly, support the drops of their liquors, and make them roll.

VIII. *Of the preservation of water in long voyages.*

It having been asked, whether, for the preservation of water in long voyages, it could not be sulphurated like wine, M. *Homburg* answered, that wine was preserved in this manner only, because the acids, which it hath naturally, being not in a sufficient quantity in proportion to the other principles, all its principles would easily disunite by the fermentation caused by the heat of the climates through which they passed, or the meer motion of the voyage; after which the wine would be no longer wine, but that the sulphur gave it new acids, which made the dose of this principle sufficient; but that there could be no room for this in water, which is spoiled only through some foreign matters which are mixed with it, and ferment, or by the eggs of worms which are hatched, these eggs being either in the
 water

water itself, or in the wood of the vessels. In this last case, a matter is requisite, which may hinder them from hatching without spoiling the water.

IX. *On a method of preserving inlaid work from the worms.*

On this occasion M. *Hombert* added, that a person of quality in *Provence*, not knowing how to preserve inlaid work from the worms destroying it in a few years, as it frequently happens in that country, he had advised him to steep his inlaid work in water wherein corrosive sublimate had been mixed, which succeeded very well.

X. *Of an excessive heat felt at Montpellier.*

M. *de Plantade* wrote to M. *Cassini* an account of the excessive heat which has been felt this summer at *Montpellier*, more especially on the 30th of *July*. There was no memory of any thing that came near it. The air was that day almost as scorching as that which comes out of the furnaces of a glass-house, and there was no place to fly from it but into cellars. In many places they baked eggs in the sun. The thermometers of M. *Hubin* broke by the liquor ascending to the top. A thermometer of M. *Amontons*, which M. *Plantade* had, though it was in a place where the air could not freely enter, rose very near that degree wherein tallow uses to melt. Most part of the vines were scorched on this day alone, which had never happened in this country. The astronomers of *Montpellier* observed, that during this burning summer, pendulums went much too fast.

At

At *Paris*, the 6th of *August* was much hotter than the 30th of *July*. A thermometer of M. *Hubin's*, which M. *Cassini* had made use of 36 years, broke about 2 of the clock: it is therefore certain, that for at least 36 years, it had not been so hot at *Paris*.

XI. *Of the effect of the burning mirror in that weather.*

Who would not have thought, that in the great heats of this summer the burning mirror of the royal palace would have produced greater effects than at any other time? but it was quite contrary, and certainly that could not have been guessed by any system. M. *Homberg* saw, that the rays of the sun, when reunited by the mirror, had hardly any strength, while the direct rays alone inflamed the air. The reason which he imagines for such a surprising *phænomenon* is, that great heat raises from the earth an infinite number of sulphurous exhalations, and that these matters, through that homogeneity which they have, according to M. *Homberg's* system, with that of light, hinder, stop, and in some sort absorb the rays, whether it be that they absolutely intercept a part of them, and hinder them from falling on the mirror, or that they have the same effect on them, which a scabbard has on a sword, and thereby take from them that extreme subtilty, which is necessary to cut hard bodies readily. This conjecture is confirmed by an experiment made by M. *Homberg*. He placed between the mirror and the *focus* a chaffindish full of live coals, so that those rays, which reached the *focus*, passed thro' the vapours of these coals, and he saw that the mirror was considerably weakened by it. This
is

is a representation of what happens in great heats, or rather, the very thing in miniature. Also M. *Homborg* has always observed, even in moderate and common heats, that when the sun has appeared many days together, the effect of the mirror is not so great, as when the sun first appears immediately after a great rain. The reason is, that this rain hath precipitated the sulphurous matters, and cleansed the air. That trembling of the light, which has always been observed with large telescopes, and which in very great gnomons makes the end of the shadow uncertain, is very naturally explained, by the system of M. *Homborg*, and is a new proof of it.

On this reflection may be made, that the burning mirror, which is a new furnace for chymists, infinitely superior to the old common furnaces, hath this inconvenience, that it cannot be but seldom made use of, at least with full force. It must be in the summer, between the hours of 9 and 3, the sun must shine, no clouds must pass over it during the whole time of the operations, the days must be of a moderate heat, and must not have been preceded by many dry days. There are some years, in which it would be difficult to find a week of such days.

XII. *Of the yellow amber; translated by Mr. Chambers.*

The marquis *de Bonac*, the king's envoy extraordinary at the court of *Sweden*, having seen some fossil amber in the lands of M. *Grata*, postmaster-general of *Prussia*, near the city of *Dantzic*, which appeared of the same nature, as that found on the sea-coast, began to consider that mixed more narrowly than before, and to doubt

whether it were really form'd of the froth of the sea, as is commonly supposed; and the cardinal primate of *Poland*, who was with him, and had the like curiosity, moved him to ask the opinion of the academy hereupon. *M. de Bonac* wrote accordingly to *Paris*, and the academy was induced hereby to collect all they could learn on this head, the result whereof was remitted to the marquis in the following memoir.

As not only the finest but the greatest quantity of amber comes from the two *Prussias*, the academy is less acquainted therewith, than those persons may be, who are pleased to consult it; yet we shall speak what we can learn of it from our own knowledge, and add some reflections thereon, without enquiring what other authors have wrote of it, as taking it for granted those authors are known, and that it is not a collection that is required of us. Mess. *Cassini* and *Maraldi*, travelling in 1700 into the the southern provinces of *France*, on account of continuing the meridian of *France*, found several mines of jet, and one of a kind of amber, in a mountain of *Languedoc* *Bugarache*, which is upwards of 27600 fathoms from the sea, and divided by other high mountains therefrom. Some take jet, as well as yellow amber, for a species of *succinum*; and 'tis certain the inhabitants of *Bugarache* make use of their amber to burn in their lamps. 'Tis much like a resin, and inferior by many degrees to the *Prussian* amber in hardness; near the mines of *Bugarache*, there are springs of salt-water, which form a little river.

In the history of the academy for the year 1700, 'tis mentioned, that there is amber found in the clefts of the most barren and naked rocks of *Provence*, which is further confirmed in the history

story for 1703^{*}; and we are assured, by the most creditable relations, that there is amber likewise found in *Sicily* along the coasts of *Agri- gentum*, *Catanea*, and *Leocata*, as also in the island of *Corfica*, and even about *Bologna* in *Italy*, and in *Umbria*, far distant from the sea; add to this, that M. *de Bonac* himself relates, that he had seen amber dug out of the grounds of M. *Grata*, which were divided from the sea by large woods and great mountains; all which seems to evince, that this matter is a natural production of the earth, nor must it be omitted, that we find little animals inclosed in amber, and that these animals are always of the terrestrial kind, as flies, pismires, &c.

However, for the greater security, it might be proper to examine, whether the terrestrial ambers have all the characters and perfections of the amber found on the sea shores; for 'tis no way impossible, but that the sea may finish the preparation of this matter, and contribute by its salt to give it the last degree of coction.

Supposing, however, that amber were always produced by the earth, at least its origin, or first formation, it remains to be enquired, whether it be of the vegetable or mineral nature.

We have never heard of any trees in *Prussia* that distil amber, or any thing like it in form of a resin; and yet it would be more natural to suppose, that the ants and flies sometimes seen in it, and which plainly confess it to have once been liquid, had been inclosed in a resin oozing from some tree, than in a mineral formed in the ground. To get over this difficulty, we must suppose that the amber had trickled from some rock, or at least that such pieces, wherein these animalcules

^{*} See pag 7 of this volume.

are found, had remained sometime fluid on the surface of the earth.

But whether we suppose amber to be vegeable or mineral, no body pretends to have ever seen it liquid, or barely soft; and yet it must have been both, and even exposed to sight at the time when the animals were buried in it.

The analysis of this substance made by the chymists of the academy, does not finally determine which class it belongs to. They have always found a very small quantity of aqueous liquor in it, which had the smell of rubbed amber, a great quantity of volatile acid salt, and store of oil, part of it whitish like water, part brownish, and part very black, according to the degrees of fire that had been given it in the distillation. The *caput mortuum* left behind was light, spongy, black, and glossy; and, being calcined by a naked fire, went mostly off in smoke, and yielded no fixed salt.

The only difference between the analyses of different ambers consists in this, that the most transparent and whitest yield more oil and volatile salt, and less *caput mortuum*, than the darker kind; which last, however, notwithstanding their store of *caput mortuum*, afforded no fixed salt.

The oil of amber has the smell of a bituminous oil, which should indicate amber to be a bitumen; but there are some resins, whose distilled oil has the same smell; and there are others, as benjoin, which yield a volatile acid salt.

But we know of none, which at the same time yields both a volatile acid salt, and an oil with a bituminous smell; whence the academy is most inclined to take amber for a bitumen, and consequently for a mineral.

'Tis

'Tis easy to see how many things are still wanting to enable the academy to form a more precise decision upon what relates to amber: it should be known,——1st, Whether in the neighbourhood of the places where the amber is dug, there be not some saline or vitriolic water. 2dly, Whether it be commonly inclosed or mixed with any earth, or other particular matter. 3dly, Whether there be any marks whereby to distinguish the places, where the amber is found. 4thly, Whether fossil amber differs in any thing from that found about the sea-shore. 5thly, Whether any white, as well as yellow amber, be found on the ground, and whether it be not the air or the sun's heat, that converts the yellow into white. 6thly, Whether in those places where yellow is dug, there be not likewise black amber found. 7thly, Whether it be true, which *Hartman* in his history of the *Prussian* amber, and *Bartholin* on the *Danish*, relate, that it is found under a kind of foliated earth, not unlike the barks of trees, and that it is accompanied with a sort of fossil wood, wherein, however, we neither perceive pith, fibres, knots, nor buttons.

XIII. Of *M. Delisle's map of the Roman empire.*

A great part of the states which at present compose the known world, were formed on the ruins of the *Roman* empire, dismembered and torn to pieces by *Barbarians*. As it is from thence that our modern histories take their original, and as it is they, which most concern us, *M. Delisle* hath prepared a map, which must be a great help to the understanding them well. It contains not only the *Roman* empire, but also all the barbarous

rous countries which surrounded it, a little time before the people of those countries had made any breach therein by their invasions. His epocha is the year 400 of the christian æra. M. *Sanfon*, a celebrated geographer, had 'already made a map of the *Roman* empire, which was very much esteemed in his time; but he did not include therein the barbarous countries, the position and determination of which must be as laborious as it is instructive. M. *Delisle* hath named his map *theatrum historicum*, because of the great extent it takes beyond the *Roman* empire, and the use it is of to our histories.

Besides the earth has been much altered since M. *Sanfon*'s time, that is, the astronomical observations both more exact and in a greater number have produced great reformatations in geography. They were much mistaken about the longitudes, which are naturally more difficult to determine than the latitudes, and were often mistaken about the latitudes themselves; and M. *Delisle* was obliged to differ always from M. *Sanfon* on the first of these measures, and often on the second, which entirely changes the figure of the countries, seas, &c.

It is an observation, which is not altogether new, that the errors in geographical measures have always hitherto consisted in the excess.

From the time of the *Greeks* to us, the earth hath always diminished each time they have undertaken to discover the bigness of it.

From thence it proceeds, that though the same *Roman* empire, or the same countries, are by M. *Sanfon* in a larger map than that of M. *Delisle*, and that consequently the scale of M. *Delisle*'s map ought to be the least, yet it is more than a fifth part larger. The reason for which is, that in
M.

M. *Sanfon's* map, that the *Roman* empire is much too large, when compared with the rest of the surface of the earth. The perfection of maps depends on the exact proportion of the parts of this surface among themselves, and we cannot hope to know it but by astronomy, which daily diffuses a greater light on geography.

AN ABRIDGMENT

OF THE
PHILOSOPHICAL MEMOIRS of the ROYAL
ACADEMY of SCIENCES at *Paris*, for
the Year 1705.

I. *Observations on the quantity of rain-water, which fell at the royal observatory during the year 1704, with the heights of the barometer and thermometer, and observations on the winds that reigned, by M. de la Hire **.

DURING the year 1704, the water which fell was pretty equally distributed through all the months, except those two of *July* and of *October*, in which it rained very little; the dry weather of this last month is very useful for the better sowing of seeds.

This is the quantity of water during each month.

	<i>Lines.</i>		<i>Lines</i>
Jan.	15	July.	9 $\frac{1}{4}$
Feb.	15 $\frac{1}{2}$	Aug.	27
Mar.	19 $\frac{1}{4}$	Sept.	34
Apr.	16	Octob.	8 $\frac{1}{4}$
May	27 $\frac{1}{4}$	Nov.	19 $\frac{3}{4}$
June	24 $\frac{1}{4}$	Dec.	23

The sum of water in the whole year 238 $\frac{1}{2}$ lines, or 19 inches 10 lines, which is very near the 19 inches, which we have determined for the mean quantity of water which falls each year.

Jan. 10, 1705.

On the winds.

Throughout the whole month of *January* the wind reigned towards the N. inclining at the beginning towards the E. and at the end towards the W. there was no rain from the 10th to the 24th.

In *February* the wind was almost always in the W. and sometimes in the S.

In *March* the wind was almost always in the S. in the beginning it inclined to the W. and at the end towards the E. there was no rain from the 15th to the 3d of the month following.

In *April* the wind was the same, except in the last days, when it turned towards the N.

In *May* the wind was very variable.

In *June*, the wind was in the beginning between the N. and E. and at the end towards the W.

In *July* the W. wind governed, and it rained no more than 4 lines from the 27th of *June* to the 28th of this month.

In *August* the wind parted from the E to N. and afterwards to the W.

In *September* the wind was almost always S. W.

In *October* the principal wind was the N. inclining towards the E. and sometimes to the W. From the 4th of this month to the 27th, it rained but one line.

In *November*, the wind was in the beginning towards the N. and in the middle and to the end towards the S. W.

In *December* the principal and governing wind was the S. W. It is seen by all these observations, that the wind which reigned most was the W. as it almost always happens in these countries; and it is also from these sorts of winds that it most

commonly rains; but the greatest rains, which did not exceed the height of an inch, came with a northerly wind. There has been no considerable storms during this year.

On the barometer.

What is most considerable in the barometer, and which shews us the weight of the air, are the changes which happen to it in 2 or 3 days, in which time we see it often rise and fall more than an inch; which makes us acquainted with the great variations which happen in a little time as to the height of the atmosphere. For to give a reason for these different weights of the air, it does not seem to me very likely to suppose, as do some philosophers, that there are different liquids and a different weight on the surface of the earth, which are sometimes carried on the one side, and sometimes on the other; for they must be commonly more light when the air is most loaded with vapours, as is known by observations.

It thinks all that is observed in the weights of the air, or of the atmosphere in all its circumstances, may be very well explained in the manner following. We know by very exact observations, that the barometer generally rises to a less height between the tropics, than in the northern countries; whence it may be conjectured that the figure of the atmosphere is a long spheroid, whose axis is joined to that of the earth, which may easily be explained in the system of *Copernicus*. But as wherever there is air there may be winds, if the same winds reign through all the mass of air, and if it comes from the S. it will lower the height of the atmosphere in these countries; and on the contrary, if it comes from the N. it will raise it. But also as the S. winds bring

bring us rain, it will follow, that it ought to rain when the air appears light, and the quite contrary will happen when it is on the other side.

This is in general what ought to follow this supposition; but if the S. wind reigns only on the surface of the earth, and that there is a N. wind in the superior part, it may rain altho' the air appears very heavy, and by a contrary reason the weather may be very serene with a N. wind, and the barometer very low; for we can only observe those winds which are near the earth.

During this year the barometer rose pretty often beyond 28 inches; but on the 25th of *December* in the morning it rose the highest to 28 inches 3 lines $\frac{1}{8}$; and the lowest was *Nov.* 25th, to 26 inches 11 lines, by the height of the great hall of the observatory where my barometer is placed. The whole difference of height between the highest and lowest has therefore been 1 inch 4 lines $\frac{1}{6}$. No conclusion can be made from the winds which have reigned in the greater or lesser heights of the barometer, for the reasons which I have above related, since we can only observe those winds which are on the surface of the earth. I have only taken notice that it did not rain when the barometer was at the highest, and that there was much rain when it was at the lowest, sometimes with a N. and sometimes with a S. W. wind.

On the thermometer.

My thermometer fell lowest on the 23d of *January* to 14 degrees $\frac{2}{3}$. Its mean state, such as it is, in the bottom of the caves of the observatory, is 14 toises below the banks level, being at 48 degrees, and the frost beginning when it is at 32 degrees; but it immediately rose a-

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gain near the 30 degrees. The heat was greatest the 28th of July, the thermometer rising to 66 degrees $\frac{1}{4}$. These observations on the thermometer are always made near the sun's rising, which is the time of day wherein the air is most cold.

It is seen thereby that the cold has been very near in the same degree as the heat, compared with a mean state, if the 23d of January is excepted. Also in the day-time, and about two in the afternoon the heat is much greater than in the morning, and I have found the thermometer at 75 degrees shaded from the sun; and consequently there has been more heat than cold this year in these countries.

On the declination of the needle.

I have observed the declination of the needle the 30th of October, 9 degrees 20 minutes towards the west, with the same needle of 8 inches length, and in the same place where I used to observe it.

II. *A comparison of the observations on the rain and winds made by M. de Pont-briant at the castle of Pont-briant, two leagues from S. Malo, and towards the sea-coast, during the year 1704; with those which were made at the observatory at the same time, by M. de la Hire *.*

These observations which have been made in Bretagne with great exactness, having been communicated to the academy by M. du Torar, to whom M. de Pont-briant had sent them; it has been thought proper to compare them with those which were made at Paris at the same time, the

* Feb. 25, 1705.

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journal of which I have already given. Here is only given the quantity of rain which fell during each month; but it must be observed, that it rained very often at the same time in these two places, which are distant about 80 leagues, one of which is westward from the other, and almost in the same parallel; but storms are much more frequent at *S. Malo* than at *Paris*.

<i>At Paris.</i>		<i>At Pont-briant</i>	
	<i>Lines.</i>		<i>Lines.</i>
<i>January</i>	15	11	$\frac{3}{4}$
<i>February</i>	15 $\frac{1}{2}$	22	$\frac{1}{2}$
<i>March</i>	19 $\frac{1}{4}$	25	$\frac{3}{4}$
<i>April</i>	16	21	$\frac{3}{4}$
<i>May</i>	27 $\frac{1}{4}$	17	
<i>June</i>	24 $\frac{1}{4}$	2	
<i>July</i>	9 $\frac{1}{4}$	13	$\frac{1}{4}$
<i>August</i>	27	27	$\frac{1}{2}$
<i>September</i>	34	51	
<i>October</i>	8 $\frac{1}{4}$	18	$\frac{3}{4}$
<i>November</i>	19 $\frac{3}{4}$	57	$\frac{3}{4}$
<i>December</i>	23	25	$\frac{1}{4}$

Sum of the water at *Paris* 238 lines $\frac{1}{2}$,
at *Pont-briant* 284 lines, or
19 inches 10 lines $\frac{1}{2}$.
23 ——— 8 ——— $\frac{1}{4}$.

It is hereby seen that the quantity of rain in each month, has not much differed, except in *September* and *November*, wherein it rained much more at *Pont-briant* than at *Paris*. And in the month of *June* it rained much less at *Pont-briant* than at *Paris*; but the one does not make amends for the other, for there fell near 4 inches more water at *Pont briant* than at *Paris*, altho' in *Paris*

the quantity was very near the same as in the mean years.

It was some years ago, that the marshal *de Vauban*, who is now president of the academy, caused the same observations to be made in the citadel of *L'Isle* in *Flanders*. I then compared them with those of *Paris*, and I found that it commonly rained a little more in *Flanders* than at *Paris*.

By the observations of the winds made at *Paris* and at *Pont-briant*, it is remarked, that the wind is not commonly the same in both places, and that it always inclines more to the S. at *Paris* than at that place. As to the rains which accompany the winds, there is much variation in the times and years. Not but that in general it is found by this year's observations, that at *Pont-briant* great rains, with storms, have been always accompanied with a N. W. wind, and sometimes a N. but seldom a N. E. At *Paris* they come almost always from the S. W. The neighbourhood of the sea at *St. Malo*, and the disposition of the *English* channel, in regard to the coast of *Bretagne*, may cause this difference, both for the direction of the wind and of the rain.

It is no wonder that the winds differ in places very little distant compared with the whole surface of the earth, since we see frequently, that in the same place there are different winds which reign in the air, and sometimes are quite opposite, which may be observed by the motion of the clouds. One of the winds may begin in one place, and the other in another, either more or less distant from the same place. These winds at length mixed make but one mean, where one gains the ascendant prevails over the other; and it may happen, that this struggle between these
contrary

contrary winds, when they are very violent, may cause storms and hurricanes.

M. de Pont-briant observes in the letter which he wrote to M. du Torar, that it freezes much less at *St. Malo* than at *Rennes*, but the cause of that ought to be attributed only to the proximity of the sea: for the great quantity of vapours, which arise from the sea-water, and may retain some marine salts, may prevent the freezing, since it is known by experience, that sea-water does not freeze so easily as fresh water, and that water, wherein a little marine salt has been dissolved, will not easily freeze. I have also formerly observed at *Brest*, that they preserved some *anana's*, or pine apples, in the open ground during the whole winter, though they were exposed to the air.

III. *Reflections on the observations of the variation of the needle, made in the pope's legate's voyage to China, in the year 1703, by M. Cassini the son *; translated by Mr. Chambers.*

A mercator's chart was lately sent us from *Pondichery*, by M. de May, a missionary, who went with M. de Tournon, the pope's legate to *China*; in which chart is expressed by dotted lines the course which the ship *Maurepas* made each day from the *Canaries*, which they left on the first of *May*, 1703, to *Pondechery*, where they arrived on the 6th of *November*, after a voyage of upwards of 6 months.

In this voyage, they observed the variation of the needle in several places by the rising and set-

* Jan, 10, 1705.

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ting of the sun, and took care to express the same on the chart, in the proper place of their rout for the day of observation.

As the new chart of the variation, drawn by Dr. Halley for the year 1700, comprehends all the places expressed in this rout, we took occasion hence to examine, whether it would agree with these new observations; and in order hereto laid down all the places, where M. de May made his observations, and Dr. Halley's chart, allowing for the different longitudes on which the two charts are constructed. — We proceed now to make a comparison.

On the 18th of May, 1703, at 358° — of longitude, and 5° — $40'$ northern latitude, the variation was found by the sun-setting to be $1^{\circ} \frac{1}{2}$ from the north towards the west. — Now the place where this observation was made, being laid down on Dr. Halley's chart, is found a little to the westward of the line of no variation, and on that side where the variation begins to be eastwards; so that by this observation it appears, that the line of no variation ought in strictness to be a little to the west of the place, wherein it appears in Dr. Halley's chart, which agrees likewise with what had been before observed in a memoir for the year 1704.

On the 6th of June at 356° — longitude, and 5° — $20'$ — southern latitude, the variation was found by the sun's rising to be 1° — eastward of the north, which agrees near enough with Dr. Halley's chart, wherein this place is represented between 1 and 2° — of variation eastward.

On the 11th of June at 352° — $40'$ longitude, and 11° — $15'$ southern latitude, the variation was found

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found $1^{\circ}—\frac{1}{2}$ eastward of the north, which in Dr. *Halley's* chart is represented as a little more than $3^{\circ}—$

On the 19th of *June* at $1^{\circ}—$ to the south of the most southern part of the island of *Ascension* being $350^{\circ}—$ of longitude, and $21^{\circ}—0'$ of southern latitude, the variation was found to be $6^{\circ}—\frac{1}{2}$ eastward of the north, where in Dr. *Halley's* chart it is represented as $7^{\circ}—\frac{1}{2}$.

On the 3d of *July* at $7^{\circ}—45'$ —longitude, and $34^{\circ}—40'$ —southern latitude, the variation was found $3^{\circ}—\frac{1}{4}$ eastward of the north, being much the same as in Dr. *Halley's* chart.

On the 8th of *July* at $24^{\circ}—10'$ — longitude, and $36^{\circ}—$ southern latitude, the variation was found $3^{\circ}—$ westward of the north, the same is represented in this place by Dr. *Halley* between 3 and $4^{\circ}—$

According to these two last observations, in one whereof the variation is found eastward, and in the other westward, both of them agreeable to what are represented in Dr. *Halley's* chart, the line of no variation crosses M. *de May's* course, much in the same place where Dr. *Halley* makes it cross.

On the 13th of *July* a degree to the south of the *Cape of Good Hope*, at $41^{\circ}—$ longitude, and $36^{\circ}—20'$ — southern latitude, the variation was found $13^{\circ}—$ westward of the north, which in Dr. *Halley's* chart is noted $11^{\circ}—$

On the 19th of *July* at $53^{\circ}—0'$ longitude, and $35^{\circ}—35'$ — southern latitude, the variation was found $19^{\circ}—$ westward of the north, exactly the same as in Dr. *Halley's* chart.

On the 25th of *July* at $69^{\circ}—$ longitude, and $32^{\circ}—50'$ southern latitude, the variation was found

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$25^{\circ} \frac{1}{2}$ westward of the north; in Dr. Halley's chart it is between 24 and 25.

On the 12th of *September* at $98^{\circ} \text{---} 30'$ longitude, and 28° southern latitude, the variation was found 19° westward of the north exactly, as represented by Dr. Halley.

On the 17th of *September* at $96^{\circ} \text{---} 35'$ longitude, and $22^{\circ} \text{---} 40'$ southern latitude, the variation was found 15° westward of the north, which in Dr. Halley's map is represented between 15° and 16° .

On the 2d of *October* at $106^{\circ} \text{---} 40'$ longitude, and $1^{\circ} \text{---} 20'$ southern latitude, the variation was found 4° westward of the north, which in Dr. Halley's chart is found between the 5th and 6th degree.

Lastly, on the 2d of *November* at $105^{\circ} \text{---} 20'$ longitude, and $14^{\circ} \text{---} 40'$ southern latitude, the variation was found $4^{\circ} \text{---} 45'$, precisely the same as in Dr. Halley's chart.

Upon the whole, it appears, that some of these observations fix the variation exactly the same, as is done by Dr. Halley, that the greatest part of them do not deviate from it a degree, and that none of them vary 2° : so near a conformity must be allowed very considerable, considering the difficulty of observing the variation with exactness at sea, and the changes which may have happened within the three years elapsed between the drawing of Dr. Halley's chart, and M. de May's voyage.

I know not whether Dr. Halley had any further view in his chart, besides determining the variation of the load-stone for the conveniency of sailors; but if in other courses and voyages, the agreement should be found as exact as in the present one, it might likewise be of use for determining

mining the longitude at sea, especially on the other side the equator; for the lines which shew the variations from degree to degree, cut the parallels in those parts pretty directly, and are likewise very near to each other, as appears in the present course from the line of no variation to that where the variation is 25° — which here correspond to 34° — difference of longitude.

In effect, almost all the places where M. *de May* observed the variation may be laid down in Dr. *Halley's* chart, by the intersections of parallels with the lines which denote the variation observed; yet without finding any other differences than what may well be attributed to the difficulty of determining the longitude of the place where the observation is made.

The variation being once well established, it were to be wished, that a rule could be found for the changes or variations which befall the same in a compass of time; but to arrive hereat, a great number of observations will be required, made with the utmost care, and at considerable distances of time, which is an advantage we are hitherto destitute of: for tho' father *Riccioli* made a large collection of such observations, yet having omitted the names of most of the observers, as well as the times when they were made, we cannot make much of them.

The chief assistance we have in this case, is from some observations made by the jesuits in their voyage to the *East-Indies*, related by father *Gouye*, in the physical observations for the year 1692, which will inform us of some changes, that the declination has undergone.

Father *Noel*, in his way to *China* in 1684, observed, that at 215 leagues westward of the *Cape of Good Hope*, the needle had no declina-

tion.— Agreeably to which observation, the line of no variation must have then been considerably to the east of the place, wherein it is represented by Dr. *Halley*, and where it ought to be now placed, according to the observations of M. *de May*, who in 1703 found the variation in the same place 3° — westward of the north.

In 1684, the same father found the variation at the *Cape des Eguilles* 10° — westward of the north, which in M. *de May*'s chart is represented as 13° — From both which observations we have an augmentation of 3° — in 19 years, which is at the rate of $10'$ — per year.

In *Ricciolus*'s collection, we have no observation of the declination at this *Cape*; which, 'tis probable had none when it was first named *Cape des Eguilles*, that is, of the needles; but in the 8th book of his geography, he gives several observations made in the neighbourhood of that *Cape*; and among others, one of *Gerard de Dieppe*, who, in 1639, found the variation 14 leagues beyond the *Cape of Good Hope*, which must be nearer the *Cape des Eguilles* 1° — $\frac{1}{2}$ westward. — By a comparison of which, with an observation given by Dr. *Halley*, we have an alteration of 11° — $\frac{1}{2}$ from the north to the westward, in the space of 64 years, which amounts to somewhat less than $11'$ — per year.

Father *Noel* further observes, that the *Portuguese* pilots affirm, that from the *Cape des Eguilles* to *Madagascar*, the declination increases 13° — westward, and consequently if it be 2 — at the *Cape*, it will be 15 at *Madagascar*, which agrees likewise with the declination expressed in the new chart, which is 13° — at the *Cape des Eguilles*, and 25° — $\frac{1}{2}$ under the meridian of *Madagascar*.

From

From *Madagascar* to *Pondichery* the declination diminishes; so that in M. *de May's* map, a little to the east of *Pondichery*, it is expressed as $4^{\circ} \text{---} 45''$ westwards of the north. Now the declination at *Pondichery* was found by father *Richaud* in 1689, to be $7^{\circ} \text{---} 0''$. From whence it appears, that the declination had diminished $2^{\circ} \text{---} \frac{1}{4}$ in 14 years, which is at the rate of $10'$ per year; whereas at the *Cape des Eguilles*, we had before found an increase of the like quantity; and it may be added, that father *Richaud* found at *Louvo*, in the space of two years, a diminution, like that here noted at *Pondichery*.—From the whole therefore it may be conjectured, that in the *East-Indies*, from the meridian of *Madagascar* eastward, the western declination diminishes every year in the same proportion, as it increases from the same island eastward.

IV. Of barometers without quicksilver for the use of the sea, by M. Amontons *.

If air is inclosed in a glass ball D †, joined to a glass tube E, C, B, A, bent at C, open at A, and containing a liquor from the entrance E of the ball, to some place of its part AB; it has been long since known that this air inclosed in D, will augment or diminish its bulk, not only in proportion as the heat of the outward air changes, but also in proportion as the weight of it changes; mean while I know not whether any one has yet distinguished and determined the quantity of these two effects, I mean, how much the heat and the weight of the outward air, acting in conjunction on that which is contained in D, will, each in their particular, make this bulk of inclosed air diminish or increase; in a word, what would be

* Feb. 11. 1705.

† Plate V. Fig. 1.

the motion of the liquor in the tube A B. These two effects have always appeared difficult to be separated from one another, because of the combination of many circumstances, which make them vary almost an infinite number of ways.

As to what concerns the effect of the weight of the atmosphere on this very air; indeed M. *Mariotte* has already given us some experiments and rules thereon; but it no where appears that his design was by this means to measure the vicissitudes of the weight of the atmosphere, by preventing us from attributing the effect of the weight to that of heat, and reciprocally that of heat to the effect of the gravity. As that may nevertheless have its use, I am endeavouring to do it in the best manner I can possibly, continuing to make use of what M. *Mariotte* has settled thereon. Now by the same experiments it is clear, that the more considerable the bulks of air in D are, the more will the liquor in the tube A B sink or rise, by one and the same over-charge, or by one and the same diminution of the weight of the atmosphere; and that if this liquor in A B, had no weight, the bulks of inclosed air would in their changes follow the proportions of the weights which they are loaded with, so that these bulks would be in an inverted ratio of these weights.

* Thus then supposing the ball D lengthened out into a long very thin cylinder of the same size with the tube A C, and the whole in a horizontal situation to avoid the weight of the liquor; if this ball thus lengthened was, for example, 14 feet long, and the liquor in E was at the beginning of these 14 feet, when the weight of the atmosphere equals 26 inches of quicksilver; this liquor would advance a foot, when the weight of the atmo-

* Fig. 2.

sphere

sphere is at 28 inches, these bulks 14 and XIII. being by the inverted ratio of the weights 26 and XXVIII; and the same change of the weight of the atmosphere would have made the liquor advance differently according as the lengthened ball had more or less capacity or length. Thus it would have advanced two feet, if the length of the lengthened ball had been 28 feet, instead of 14; and 4 feet if this lengthening had been 56 feet; and so of the rest. Whence it appears that the effect of the weight of the atmosphere on the air in the ball D, becomes always greater and greater, according as the greatness of this ball increases; which cannot be said of the effect of heat, which, as I have already shewn elsewhere, would be always equal, notwithstanding the augmentation of these bulks. The ball D may then be supposed so prodigiously great, that the effect of the changes of the heat of the atmosphere, should not be discerned in comparison of the effect of the changes of its weight; which still supposes the tube A B to be in a horizontal situation. But as such a situation is inconvenient for use, and that it would be better to have it vertical, in this situation the liquor cannot pass from the tube A C, into C D, or out of that into the other, without lessening the impression of the weight of the atmosphere, against the air inclosed in D, or of this against the atmosphere; and that so much the more according to the greater weight of the liquor which shall be made use of. Thus, for example, if the weight of this liquor is to that of the quicksilver as 1 to 14, and if a quantity of this liquor contained in A B, in the compass of 28 inches, passes into the ball D toward E, it is plain that this sinking of 28 inches of liquor will equal the effect of
of

of the atmosphere, whose weight will be encreased to a quantity equal to two inches height of quicksilver; and as we know experimentally, that the motion of the simple barometer, which is caused by the more or less weight of the atmosphere, does not here exceed the extent of two inches; it is also evident, that the range of the liquor in the tube A B, situated vertically, cannot by the change of the weight of the atmosphere, exceed with a like liquor 28 inches, let the ball D be never so large: it cannot even, taking it rightly, go so far, because the spring of the air in D will always make some resistance to the diminution of its bulk, let this diminution be never so little; and let the tube A B be never so small, and consequently how little soever the quantity of liquor is, which is contained in the compass of 28 inches of this tube, it is impossible that this quantity of liquor passing from A B into E, should not something diminish the bulk of air in D. The compass of this range of the liquor in the tube A B, will be even considerably less than 28 inches, tho' the ball D should be only of a moderate size; and experience has shewn me, that with a liquor, whose weight is to that of quicksilver, near as 1 to 14, the extent of this range can be but 20 inches, with balls of two inches diameter; and only 16 inches, with balls of 1 inch $\frac{1}{4}$; which would still be less if the liquor was more heavy. But as on the contrary, those which are more light, may very well be employed in it; and as already this range of 20 inches is at least as considerable as that of the double barometer of M. *Huygens*; nothing hinders the tubes A C D from being serviceably made use of, in which there must be liquor from the middle of the part A B to E, to know by the motion of the

the liquor in A B, the changes of the atmosphere, in the same manner as is done with common barometers ; and so much the better, as being more portable, and not so susceptible of motion, they may very well be made use of at sea, where the shaking of the vessel will not hinder an exact observation of the diverse changes ; which cannot be done with the common ones. Having found that the extent of the range of the liquor in these tubes by the changes alone of the weight of the atmosphere, were considerable enough to make use of it as a barometer ; and after having divided this extent into 24 equal parts, to make thereof a graduation, which may describe the quantities of quicksilver, which equal the weight of the atmosphere in all its changes ; it now remained for me to apply this graduation to these new barometers. This did not at first appear to me very easy, because of the action of heat, which continually changing, did not permit me to be able to assign on these tubes any fixed place for this graduation. But having considered that even this which seemed an obstacle to me, might serve me for a direction, this graduation being always to follow the motion which heat should cause in the liquor, and that when heat caused no motion in it, this graduation ought also to remain in the same place ; I made it moveable in the following manner.

I put one of these tubes for a considerable time near one of my thermometers, and I observed the range of both, at times when I was assured by the observation of the barometer, that the weight of the atmosphere had not changed ; which gave me the means of making on the side of this tube a graduation, like those of my thermometers, though greater. This graduation described the changes which heat caused

in the height of the liquor of this tube. After this I put on the side of this graduation of the effect of the heat, the graduation which I had first made of the effect of the weight of the atmosphere; so that I could raise it or lower it as I thought fit, and bring the middle of it to such a degree with that of the heat as I pleased; and when I would know the weight of the atmosphere, I looked first to see what degree my thermometer was at, and afterwards I carried the middle of the graduation of the barometer to the same degree of that which I had made on the side of the tube, to describe the changes caused by heat on the liquor of the tube, which then shewed me by the moveable graduation, the weight of the atmosphere which I sought after.

Having afterwards proved these observations for a considerable time on my rectified barometer, I can affirm that I have always found both of them exactly the same. This will more easily be believed, if it is considered that no quicksilver enters into the construction of these new barometers, and that the heat acts very weakly on the liquor which they contain, which is also but in a very little quantity; which is the reason why these barometers must be exempt from those defects which I have observed in the common ones where quicksilver is employed. It is true that the graduation of these new barometers, which are to include the effect both of heat and of the weight of the atmosphere, obliges them to be made of more than ordinary height; but this cannot go so far as to make them useless; those whose balls have two inches diameter, being not requisite to be more than 5 feet long, and the others only 4 feet, which is but about 10 inches more than the common ones when they

they are mounted; and this ought not to hinder the endeavours of making them some way useful to navigation by the observations which may be made by them at sea.

V. *Reflections on the rules for the condensation of the air, by M. Cassini the son* * ; translated by Mr. Chambers.

In our travels for continuing the meridian of Paris, we observed the height of several mountains by the edge of the sea, and among others that of *Puy de Dome*, where M. *Perier* made observations of the height of the mercury, mentioned in M. *Paschal's* treatise of the equilibrium of fluids, which observations having served M. *Mariotte* for confirming his rules of the condensation of air, I took occasion to compare his rules with my own observations.

M. *Mariotte*, in his second essay of the nature of air, gives us some experiments, which he made to determine the condensation of air; from whence he infers, that it may be taken for a certain rule or law of nature, *that the density of air is in proportion to the weight wherewith it is pressed.*

On this principle he afterwards determines in a very ingenious manner, the height of the atmosphere to be about 15 leagues of 2000 fathoms each.

He supposes that mercury in its natural state, on a level with the sea, is sustained in a barometer at the height of 28 inches, which make a balance to the whole column of the atmosphere, and that in this case a line of mercury sustains 60 feet of air, and the 12th part of a line 5 feet.

* March 24, 1705.

If we suppose the mercury removed into a higher place, where it is only suspended to the height of 14 inches, it will not sustain above $\frac{1}{2}$ the weight of the atmosphere; and consequently the air, which, according to M. *Mariotte's* rule, condenses in proportion to the weight wherewith it is pressed, should here be rarer by $\frac{1}{2}$, than in the former case; and a line of mercury, which on a level with the sea, sustains 60 feet of air, will here only sustain 120 feet, and a 12th part of a line 10 feet.

Tho' the increase of each 12th of a line, adds M. *Mariotte*, may be found by the same rules whereby we find the logarithms; but in regard the sum of geometrical progressions does not differ much from the sum, found by taking those progression arithmetic proportions, I here make my *calculus* according to the latter proportion; and to find the sum, take 7 and $\frac{1}{2}$, as an arithmetical mean between 5 and 10, which I multiply by 2016 12ths of lines, that is by 14 inches, the product 15120, or 2520 fathoms, will be the whole length of air, from a place of observation on a level with the sea, to a place where the air is only $\frac{1}{2}$ that weight, that is, to a place where the mercury is only sustained 14 inches high.

By the same method, M. *Mariotte* afterwards determines the rest of the height of the atmosphere; and to confirm this *calculus* of the height of the air, applies it to two celebrated observations, one whereof is delivered in M. *Pascal's* treatise abovementioned; and the other made some years ago, by M. *Cassini*, which is as follows.

He took the height of a mountain in *Provence*, which is on the edge of the sea, and found it 1070 feet; the mercury in the barometer which he

he used, stood at 28 inches at the foot of the mountain, and at the top thereof sunk 16 lines $\frac{1}{3}$.

In examining this observation, M. *Mariotte* makes use of an arithmetical progression, according to which, supposing that 63 feet height of air correspond at a level with the sea to 1 line of mercury, he finds that the height, where the mercury is to sink 16 lines $\frac{1}{3}$, is 1080 feet, which comes very near the 1070 feet, observed by M. *Cassini*.

As the arithmetical proportions used by M. *Mariotte*, in examining these observations, are not intirely conformable to the geometrical progressions, resulting from his rule of the condensation of the air, which however inconsiderable in little heights, may occasion very sensible differences in great ones, I have formed a table upon M. *Mariotte*'s principals, wherein the height of air is expressed, which corresponds to each line's decrease of height of mercury, from the surface of the sea upwards. In this table I have supposed with M. *Mariotte*, that the mercury is sustained to the height of 28 inches on a level with the sea, and that in this case 63 feet high of air corresponds to a line of mercury.

By this table it appears, that when the mercury sinks 16 lines $\frac{1}{3}$, the height of air corresponding to the last line, is 66 feet, 1 inch, 9 lines, and the height of the place of observation from the surface of the sea 176 fathom, 5 inches, 7 lines, or 1056 feet, 5 inches, 7 lines, which is less by 23 feet, than what M. *Mariotte* had determined by his arithmetical progression. A proof that his manner of examining observations differs considerably from the principals he himself has established, which will appear still more visibly in the observations I have to relate hereafter, which were made at much greater heights.

If

If in lieu of taking 63 feet for the height of air corresponding to a line of mercury on a level with the sea, we suppose it 60 feet, as M. *Mariotte* does for determining the height of the atmosphere, we should have for the 16 lines $\frac{1}{3}$ decrease of the height of the mercury only 1005 feet height of air, which is much less than was found on the preceding supposition; but as it would be easy to reconcile this observation of my father with M. *Mariotte's* rule, by only supposing the height of air at the surface of the sea somewhat greater than what he has laid down, we proceed to examine the second observation, which is that of M. *Pascal*, and which he endeavours likewise to reconcile to his principals.

The other observation, says he, was made in a high mountain near the city *Clermont* in *Auvergne*, the principal circumstances whereof were these.—The mercury in the barometer at the bottom of the mountain stood at 26 inches, 3 lines $\frac{1}{2}$; when carried 27 fathoms high, it sunk to 26 inches, 1 line; at 150 fathoms, it sunk to 25 inches; and lastly, towards the top of the mountain, which was 500 fathoms above the first place, the mercury stood at 23 inches, 2 lines. The first observation shews us, that the lowest place near *Clermont* is much higher than the vaults of the observatory, and consequently that a line of mercury here must be equivalent to above 63 feet. It may be calculated thus:

The difference between 26 inches, 3 lines $\frac{1}{2}$, and 28 inches, is 20 lines $\frac{1}{2}$, and according to the *calculus* above declared, the last division must increase it about 7 feet, above 63, for the produce of 63 by 21, divided by 168, gives somewhat above 7 feet, which added to 63, gives 70 feet; supposing therefore that the first line of

mercury were then equivalent to 70 feet of air, reckoning from the lowest place of observation, M. *Mariotte* finds the height of the place of the last observation to be 2940 feet, or 490 fathoms of air.

In examining this observation, M. *Mariotte* makes use of two arithmetical progressions: by the first, he finds that at a decrease in the height of mercury of 20 lines $\frac{1}{2}$, the height of air corresponding to the last line of the mercury is 70 feet; whereas according to the table, it should only be 67 feet; so that between the height of air corresponding to a line of mercury on a level with the sea, and that arising from his *calculus*, when the mercury is sunk 20 lines $\frac{1}{2}$, he finds by his arithmetical progression a difference of 7 feet instead of 4 feet, which arise by the geometrical progression, so that the error makes it almost double.

He would easily have discovered these differences, if agreeably to his rule he had made, as to 28 inches height of mercury on a level with the sea, are to 26 feet, 3 lines $\frac{1}{2}$ height of mercury in the lowest place at *Clermont*, so are 63 feet height of air on a level with the sea to 67 feet, 1 inch height of air, which correspond to a line of mercury in the lowest part of *Clermont*.

The second progression which he uses, afterwards carries him still farther from the truth, but not to enter into too long detail, it may suffice to compare what arises from this progression, with what is expressed in the table drawn upon his own principals; it will here appear, that as the height of mercury was 20 lines $\frac{1}{2}$ below the standard at *Clermont*, the height of that city above the surface of the sea must be 222 fathoms, 2 feet, 1 line, or 1334 feet, that the mercury
having

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having sunk 37 lines $\frac{1}{2}$ between *Clermont* and the top of *Pui de Dome*, which amounts to 58 lines in all from the surface of the sea, the height of that mountain must be 670 fathom above the surface of the sea; from whence subtracting 222 fathoms, the height of *Clermont* above the same surface, we have 448 fathoms for the height of *Pui de Dome* above *Clermont*; whereas M. *Mariotte* had made it by his *calculus* 490 fathoms, and M. *Pascal* 500 fathoms.

It will hereafter appear, that the height of *Pui de Dome* above *Clermont* is upwards of 500 fathoms, and consequently differs still farther from the 448 fathoms, which arise from the principals of M. *Mariotte*.

It may here be proper to enquire, whether the observations, since made by M. *de la Hire* on the mountain *Clairret* in *Provence*, and those made by myself in my expedition for continuing the meridian, agree with the principals of M. *Mariotte*. — M. *de la Hire* found upon the mountain *Clairret* the height of the mercury to be 26 inches, 4 lines $\frac{1}{2}$; and the like operation being repeated at the edge of the sea 3 hours after, the mercury was found 28 inches, 2 lines high, the difference therefore is 1 inch, 9 lines $\frac{1}{2}$, and the height of the rock was found by mensuration to be 277 fathoms.

Now by the table calculated on M. *Mariotte's* principals, the height of air corresponding to 1 inch, 9 lines $\frac{1}{2}$, is 233 fathoms, 3 inches, which is less by 23 fathoms $\frac{1}{2}$, than what M. *de la Hire* found it by his observations.

One of the most accurate observations I made in continuing the meridian, was on the tower *de la Massana*, near *de Collioure*. The distance of this tower, from the place where we observed its height,

height, was determined by the triangles of the meridian line. The height of the place of observation at *Collioure*, above the surface of the sea, was measured very exactly with a cord, and the angle of height taken with a very good instrument, was above 7 degrees ; so that an error of a minute in the observation would not have occasioned an error of a fathom in the determination of the height.

This height was further confirmed by an observation made from the tower *de S. Elme*, whose height from the surface of the sea, was exactly known by the first observation, we determined the height of this tower above the place of observation at *Collioure* to be 397 fathoms, and above the surface of the sea 408 fathoms.

On the 12th of *March*, when the barometer at *Collioure* stood at 28 inches, we carried it to the foot of the tower *de la Massane*, where we found the mercury sunk to 25 inches, 5 lines, the difference is 2 inches, 7 lines, which correspond to 397 fathoms ; looking in the table for the height of air corresponding to 2 inches, 7 lines, we find it 342 fathoms in lieu of 397 found by the observation.

Hence it appears, that the heights arising from the principal of M. *Mariotte*, do not agree with observation, but recede the further therefrom, as the distances are greater ; for there is no room to ascribe this difference of 55 fathoms, found between these heights, to any error that could have crept into the mensurations of the heights of the mountain and mercury, those observations having been made with all the exactness imaginable.

We took the height of *Bugarache*, a mountain of *Languedoc* near the sea-coast, in three different manners, and found it 648 fathoms.

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The height of the mercury was here found on the 15th of *January* two hours after noon, to be 23 inches, 8 lines $\frac{1}{2}$. At *Paris*, it was found on the same day, at 7 in the morning, 27 inches, 3 lines $\frac{1}{4}$, and it sunk in the course of the day about $\frac{1}{2}$ a line; so that we may suppose it 27 inches, 3 lines, and by adding thereto the 4 lines corresponding to the 40 fathoms height of the observatory above the surface of the sea, we shall have the height of the mercury on a level with the sea, 27 inches, 7 lines, which is more than what we found at *Bugarache*, by 3 inches, 10 lines $\frac{1}{2}$.

In the table, we find that the height of air corresponding to 3 inches, 10 lines $\frac{1}{2}$, is 527 fathoms, which is less by 121 fathoms than the 648 fathoms, found by the observation of the height of the mountain. Could the height of the mercury at the brink of the sea, have been observed at the same time that we observed it on the mountain, nothing could have been further required for the exactness of the observation; but this we could not do because otherwise employed.

The two most considerable observations after these, were those of two mountains of *Auvergne*, near the mount *d'Or*; one whereof is called *la Coste*, and the other *la Courland*. On the first of these, which is 851 fathoms above the surface of the sea, we found the height of the mercury on the 9th of *October*, 3 hours after noon, to be 23 inches, 4 lines. At *Paris*, it was found at 5 in the evening, 27 inches, 10 lines, which is higher by 4 inches, 6 lines, than on the top of the mountain.

On the 12th of *October*, about noon, we find on the top of the *Courlande*, which is 838 fathoms

thoms above the sea, the height of the mercury to be 23 inches, 4 lines. At *Paris*, it was 27 inches, 10 lines, which is higher by 4 inches, 6 lines, than on the top of the mountain, agreeably to what we found on the mountain *de la Coste*. This difference should have been somewhat less, by reason that the height of the *Courlande* is not so great as that of *la Coste*; but there is no hopes of arriving at a greater exactness, it being impossible but some error must arise, both in the observation of the heights taken with instruments, and in those of the barometer, observed in two different places; adding 4 lines, which correspond to the height of the observatory, to 4 inches, 6 lines difference, between the heights of mercury found at the same time on these mountains and at the observatory, we shall have 4 inches, 10 lines for the difference of the height between the surface of the sea and the top of these mountains, which we may suppose 844 fathoms, by taking a medium between the two observations.

For these 4 inches, 10 lines, the table gives us 669 fathoms high in lieu of 844 fathoms found by observation, which makes a difference of 175 fathoms. This is too considerable, to be owing to any error in the observations; for tho' the mountain be at a distance from the sea, yet its height is found exactly enough, without any great number of operations.

It remains to examine the result of the observation made on the *Pui de Dome*, whose height above the surface of the sea, was found 810 fathoms.

It were to be wished, that while M. *Perier* observed the mercury on this mountain, and that of *Clermont*, the like had been done at *Paris*, whose height above the surface of the sea is precisely

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known; but this defect may be supplied by some observations of the greatest and least height of the mercury at *Paris* and *Clermont*, related in a letter of M. *Perier*, inserted in the treatise of the *equilibrium* of fluids.

At *Clermont*, the highest was 26 inches, 11 lines $\frac{1}{2}$, Feb. the 14th, 1651.

At *Paris*, the highest was 28 inches, 7 lines, Nov. the 3d and 5th, 1649.

The difference is 1 inch, 7 lines $\frac{1}{2}$.

At *Clermont*, the lowest was 25 inches, 8 lines, Oct. the 5th, 1649.

At *Paris*, the lowest was 27 inches, 3 lines $\frac{1}{2}$, Oct. the 4th, 1649.

The difference is 1 inch, 7 lines $\frac{1}{2}$.

The difference between the greatest height of the barometer at *Paris* and at *Clermont*, is 1 inch, 7 lines $\frac{1}{2}$; the same which is found between the barometer at those two places when the lowest, which seems to intimate, that there is nearly the same constitution of air in both places.

If this difference therefore be supposed the same that corresponds to the difference of height between *Clermont* and *Paris*, and that the place of observation at *Paris* be 25 fathoms above the level of the sea, which correspond to 2 lines $\frac{1}{2}$ of mercury, we shall have 1 inch, 10 lines of mercury, for the height of *Clermont* above the surface of the sea.

Now for 1 inch, 10 lines, the table gives us 239 fathoms for the height of *Clermont* above the surface of the sea, which subtracted from 810 fathoms, the height of *Pui de Dome* above the surface of the sea, leaves 571 fathoms for the height of *Pui de Dome* above *Clermont*, in lieu of 500 fathoms, which M. *Perier* supposes of 490 fathoms, which M. *Mariotte* reduces by his *calculus*,

culus, and of 448 which follow from his principles, all the observations hitherto related conspire in making the dilatation of the air, as we recede from the earth, greater than what agrees with the principals of M. *Mariotte*, it even seems, as if in the two observations compared by him with his rules, he had been aware of this difficulty, and had been induced thereby to depart in some measure from his rules, and make use of an arithmetical progression, which he supposes not to differ sensibly from the geometrical one; tho' we have shewn, that the difference is in reality very considerable.

The height of the air, which arises from M. *Mariotte's* rules, deviating so far from the observations above related, 'tis no wonder, it should not agree with what M. *Maraldi* has established, which is founded on experiment, and exhibits all our observations with tolerable exactness; they may easily be compared together, the heights of air arising from M. *Mariotte's* rule being placed in the table against those arising from our observations, it will here appear, that at 5 inches fall of mercury, the height of air corresponding to a line of mercury, should be 20 fathoms, which is rarer by $\frac{1}{2}$ than in a level with the sea, in lieu of 12 fathoms, 4 inches, 8 lines, which arise from the rules of M. *Mariotte*, &c.

It will be difficult likewise to reconcile the consequences flowing from his experiments, with his reasonings.—For instance, first, that if luke-warm water be placed $\frac{1}{4}$ of a league high, it will boil; since in the receiver of an air-pump it boils vehemently, when $\frac{1}{2}$ the air is exhausted. And, 2dly, that if there were a mountain a league and a half high, men, or even birds, could not live thereon, by reason their blood being only pressed
by

by half the weight of air, and being hotter than luke-warm water, would yield abundance of air-bubbles, which would prevent its circulation, and disturb the natural œconomy of the heart, and other parts.

For, according to our observations, the height of air corresponding to a line of mercury at 844 fathoms height, is 19 fathoms, 3 feet, which is somewhat less than double the height corresponding to a line of mercury on a level with the sea, and yet we perceived no inconvenience from the rarefaction of the air, if we suppose the dilatation of the air to persist in the *ratio* above-established from experiment, the height of air corresponding to a line of mercury on the *Canigou*, which is 1450 fathoms, or $\frac{3}{4}$ of a league high above the surface of the sea, will be found 24 fathoms; but tho' we were only to suppose it a little above 20 fathoms, this should suffice to produce all the effects mentioned by M. *Mariotte*; and yet several persons have been on the mountain, and a pyramid was even erected by his majesty's command on the highest part of it, for the convenience of our observations, yet we do not hear of any such accident befalling them.

A table of the height of air which answers to the height of the quicksilver in the barometer.

Falling of the quicksilver.				The air's height answering to each line of quicksilver according to M. Mariotte.				The air's height upon the surface of the sea according to M. Mariotte.				The air's height which answers to each line of quicksilver according to our observations.				The air's height upon the surface of the sea according to our observations.				Height of the quicksilver.	
In. L.				T. F. In. L.				T. F. In. L.				T. F.				T. F.				In. L.	
0 0				10 3 0 0				0 0 0 0				10 0				0 0				28 0	
1				10 3 2 3				10 3 2 3				10 1				10 1				11	
2				10 3 4 6				21 0 6 9				10 2				20 3				10	
3				10 3 6 10				31 4 1 7				10 3				31 0				9	
4				10 3 9 1				42 1 10 8				10 4				41 4				8	
5				10 3 11 4				52 5 10 0				10 5				52 3				7	
6				10 4 1 9				63 3 11 9				11 0				63 3				6	
7				10 4 4 1				74 2 3 10				11 1				74 4				5	
8				10 4 6 5				85 0 10 3				11 2				86 0				4	
9				10 4 8 10				95 5 7 1				11 3				97 3				3	
10				10 4 11 2				106 4 6 3				11 4				109 1				2	
11				10 5 1 7				117 3 7 10				11 5				121 0				1	
1 0				10 5 4 0				128 2 11 10				12 0				133 0				27 0	
1				10 5 6 5				139 2 6 3				12 1				145 1				11	
2				10 5 8 10				150 2 3 1				12 2				157 3				10	
3				10 5 11 4				161 2 2 5				12 3				170 0				9	
4				11 0 1 9				172 2 4 2				12 4				182 4				8	
5				11 0 4 3				183 2 8 5				12 5				195 3				7	
6				11 0 6 9				194 3 3 2				13 0				208 3				6	
7				11 0 9 3				205 4 0 5				13 1				221 4				5	
8				11 0 11 10				216 5 0 3				13 2				235 0				4	
9				11 1 2 4				228 0 2 7				13 3				248 3				3	
10				11 1 4 11				239 1 7 6				13 4				262 1				2	
11				11 1 7 7				250 3 3 1				13 5				276 0				1	
2 0				11 1 10 2				261 5 1 3				14 0				290 0				26 0	
1				11 2 0 9				273 1 2 0				14 1				304 1				11	
2				11 2 3 4				284 3 5 4				14 2				318 3				10	
3				11 2 6 0				295 5 11 4				14 3				333 0				9	
4				11 2 8 8				307 2 8 0				14 4				347 4				8	
5				11 2 11 4				318 5 7 4				14 5				362 3				7	
6				11 3 2 1				330 2 9 5				15 0				377 3				6	
7				11 3 4 10				342 0 2 3				15 1				392 4				5	
8				11 3 7 7				353 3 9 10				15 2				408 0				4	
9				11 3 10 4				365 1 8 2				15 3				423 3				3	
10				11 4 1 1				386 5 9 3				15 4				439 1				2	
11				11 4 3 11				388 4 1 2				15 5				455 0				1	
3 0				11 4 6 9				400 2 7 11				16 0				471 0				25 0	

Falling

Falling of the quicksilver.			The height of the air answering to each line of the quicksilver according to M. Mariotte.			The height of the air upon the surface of the sea according to M. Mariotte.			The height of the air which answers to each line of quicksilver according to our observations.			The height of the air upon the surface of the sea according to our observations.			Height of the quicksilver.						
In.	L.		T.	F.	In.	L.	T.	F.	In.	L.		T.	F.	In.	L.		T.	F.	In.	L.	
3	0		11	4	6	9	400	2	7	11		16	0		471	0			25	0	
	1		11	4	9	6	412	1	5	5		16	1		487	1				11	
	2		11	5	0	4	424	0	5	9		16	2		503	3				10	
	3		11	5	3	3	435	5	9	0		16	3		520	0				9	
	4		11	5	6	2	447	5	3	2		16	4		536	4				8	
	5		11	5	9	1	459	5	0	3		16	5		553	3				7	
	6		12	0	0	0	471	5	0	3		17	0		570	3				6	
	7		12	0	2	11	483	5	3	2		17	1		587	4				5	
	8		12	0	5	11	495	5	9	1		17	2		605	0				4	
	9		12	0	8	11	508	0	6	0		17	3		622	3				3	
	10		12	0	11	11	520	1	5	11		17	4		640	1				2	
	11		12	1	2	11	532	2	8	10		17	5		658	0				1	
4	0		12	1	6	0	544	4	2	10		18	0		676	0			24	0	
	1		12	1	9	1	556	5	11	11		18	1		694	1				11	
	2		12	2	0	2	569	2	9	1		18	2		712	3				10	
	3		12	2	3	3	581	4	3	4		18	3		731	0				9	
	4		12	2	6	5	594	0	9	9		18	4		749	4				8	
	5		12	2	9	7	606	3	7	4		18	5		768	3				7	
	6		12	3	0	9	619	0	8	1		19	0		787	3				6	
	7		12	3	3	11	631	4	0	0		19	1		806	4				5	
	8		12	3	7	2	644	1	7	2		19	2		826	0				4	
	9		12	3	10	5	656	5	5	7		19	3		845	3				3	
	10		12	4	1	9	669	3	7	4		19	4		865	1				2	
	11		12	4	5	0	682	2	0	4		19	5		885	0				1	
5	0		12	4	8	4	695	0	8	8		20	0		905	0			23	0	
	1		12	4	11	8	707	5	8	4		20	1		925	1				11	
	2		12	5	3	0	720	4	11	4		20	2		945	3				10	
	3		12	5	6	5	733	4	5	9		20	3		966	0				9	
	4		12	5	9	10	746	4	3	7		20	4		986	4				8	
	5		13	0	1	4	759	4	4	11		20	5		1007	3				7	
	6		13	0	4	9	772	4	9	8		21	0		1028	3				6	
	7		13	0	8	3	785	5	5	11		21	1		1049	4				5	
	8		13	0	11	10	799	0	5	9		21	2		1071	0				4	
	9		13	1	3	4	812	1	9	5		21	3		1092	3				3	
	10		13	1	6	11	825	3	4	0		21	4		1114	1				2	
	11		13	1	10	6	838	5	2	6		21	5		1136	0				1	
6	0		13	2	2	2	852	1	4	8		22	0		1158	0			22	0	
	1		13	2	5	10	865	3	10	9		22	1		1180	1				11	
	2		13	2	9	6	879	0	8	0		22	2		1202	3				10	
	3		13	3	1	3	892	3	9	3		22	3		1225	0				9	
	4		13	3	5	2	906	1	2	5		22	4		1247	4				8	
	5		13	3	8	9	919	4	11	2		22	5		1270	3				7	
	6		13	4	0	7	933	2	11	9		23	0		1293	3				6	
	7		13	4	4	5	947	1	4	2		23	1		1316	4				5	
	8		13	4	8	3	961	0	0	5		23	2		1340	0				4	
	9		13	5	0	1	974	5	0	6		23	3		1363	3				3	
	10		13	5	4	0	988	4	4	6		23	4		1387	1				2	
	11		13	5	8	0	1002	4	0	6		23	5		1411	0				1	
7	0		14	0	0	0	1016	4	0	6		24	0		1435	0			21	0	
	1		14	0	4	0	1030	4	4	6		24	1		1459	1					
	2		14	0	8	0	1044	5	0	6		24	2		1483	3					

VI. *That the experiments, used to prove that fluids are first condensed and cooled before they are dilated, at the approach of heat, do not prove it; and that this apparent condensation is merely owing to the dilatation of the glass and of the vessels, which contain these fluids, by M. Amontons *; translated by Mr. Chambers.*

Tho' reasonings, founded on experiment, should always be the surest and most just, yet we are frequently betrayed into errors by means thereof. At a meeting of the academy on the 12th of last November, I shewed, that a glass bottle, terminated with a narrow neck or tube, being filled with water to about $\frac{1}{2}$ the height of the tube, and the hands applied thereon, the heat of the hand sunk the water in the tube before it made it rise.

And M. Geoffroy, in a memoir upon cold dissolutions, relates a parallel fact; putting, says he, some cold water in a large basin, I plunged a glass cucurbit full of equally cold water in the middle thereof, and put a very active thermometer in the cucurbit, then casting 4 or 5 shovels of lighted coals into the basin, the liquor in the thermometer instantly sunk 2 or 3 lines, but rose again in a few moments, &c.

In my physical remarks and experiments, &c. printed in 1694, speaking of 2 thermometers, one whereof was full of *aqua secunda*, and the other of spirit of wine; I observed, that having applied my hand upon the former thermometer, I found it first sink upwards of a line, after

* Mar. 18, 1705.

which it rose again very considerably, while the other thermometer, which I held in the other hand, arose at once, without any other fall of its liquor at all.

But before any of this, *Borelli* and *Isaac Vossius*, the first, in his treatise of percussion, and the other, in his treatise of the motion of the winds and sea, mention the like experiments; *fiat*, says *Borelli* *, *phiala vitrea A B C, ejusque fistula tenuissima A B, impleaturque aqua vel quolibet alio fluido usque ad terminum D: si postea eadem phiala immergatur intra vas E F G H, aqua calida plenum subito aqua deprimitur a signo D usque ad O, & e contra si immergatur intra aquam glaciale subito aqua sublevatur usque ad signum I.* As to *Isaac Vossius*, his way of representing the experiment is thus: Take a glass bottle with a broad belly and a narrow neck, fill it full of cold water, and plunge it in hot or barely lukewarm water; after the first shrinking, which only holds a moment, and which, upon the sudden contact of the hot water, makes the cold sink a little, it quickly rises again, but if you heat the water in the bottle ever so little, and plunge it in cold water, the contrary will be found.

Now tho' each of these experiments have something peculiar to them, which shews, that they had all been made separately, yet they agree in this, that the liquor sinks first before it rises, upon the approach of heat, which could not be, unless either the capacity of the bottle were increased, or the liquor contained in it condensed, or both the one and the other, which has given rise to two different opinions.

* Plate V. Fig. 3.

Vossius and *M. Geoffroy*, hold for the condensation of the liquor, and *Borelli*, to whom I likewise adhere, for the dilatation of the glass; in effect, the former system, which appears only like a paradox, will perhaps be found at bottom a meer paralogism, it being utterly inconceivable how heat should condense a liquor, which makes so obstinate a resistance against all compression, as common water is known to do; the most plausible way of solving it would be, by alleging, that the fiery particles, diffused thro' all bodies, both solid and fluid, have a tendency to re-unite, where they happen to be, in the greatest quantity, which makes them relinquish for a little time, those places where they are fewer—but till there be some experiment to support this reasoning, it will have no great force.

By the way, it being of the last importance, if we would extend our knowledge, not to admit any false principal, and there being but too much bias in our nature, to what seems surprising, it may not be amiss to make a strict scrutiny of these opinions, in order whereto I argue thus :

If the seeming condensation of the fluid, upon the approach of heat be real, its effect will be more sensible, the more susceptible of condensation the liquor made use of is ; and on the contrary, if it be the bottle that enlarges its capacity, the effect will be the less sensible, as the liquor made use of is more liable to be condensed, inasmuch as it cannot have this quality, without at the same time having its opposite one, *viz.* rarefaction, which of course will undo the effect of the augmentation of the bottle more readily, than a fluid which rarefies with less ease ; and this is what we actually find ; for in the experiment above related of the two thermometers, one filled

M m 2

with

with *aqua secunda*, and the other with spirit of wine; tho' my two hands were heated as equally as possible, I perceived no condensation in the spirit of wine before it rose, tho' there was a very sensible one in the *aqua secunda*; and this, notwithstanding that the bowl of the former was 12 times less than that of the latter, where, if the liquor had really condensed upon the approach of heat, so small a body must certainly have felt the impression sooner than a greater one; for notwithstanding its smallness, its dilatation was 6 times greater than the large one, so that there could be no reason to hinder the spirit of wine from condensing more considerably than the *aqua secunda*, provided the condensation had really taken place; and hence we may safely infer, that 'tis the dilatation of the glass, which by increasing the capacity of the bowls, produces this appearance of condensation in the fluid, and that it can no longer be urged, as is done by *Vossius*, that heat condenses fluids, before it dilates them, any more than that such fluids are colder at this time, than before the heat came near them; an opinion which has nothing in nature to keep it in countenance.

Tho' this experiment might have sufficed to shew, that those already related, do not prove the cooling or condensation of fluids, upon the approach of heat, I have further confirmed it by another.

Thrusting the glass tube * A B, thro' the cork C, which stops a bottle D E, about 3 inches in diameter, and 4 inches high, almost to the bottom of the bottle, till the tube dipped in a little water deposited therein, while the rest of the

* Fig. 4.

bottle

bottle was only full of air, which raised the water of the tube to F. 2 or 3 inches above the neck of the bottle.

Now every body knows, that air readily receives the impression of cold, and that we have no nicer thermometers than those made in this manner, and yet applying both my hands upon the bottle, the water in the tube did not sink above 2 or 3 lines, and even upon repeating the experiment several more times, it did not sink at all, but rose quickly to the top of the tube, whereas, when the bottle is quite full of water, the descent of the water in the tube A B, by the warmth of the hand, is upwards of $\frac{1}{2}$ an inch. I might easily have repeated these experiments, with other degrees of heat, more considerable than those of the hand, but this seems unnecessary.

E're we conclude, it may be observed, that by these words of *Borelli*, *impleaturque aqua vel quolibet alio fluido*, it appears, that tho' he really ascribed the sinking of the water to the dilatation of the bottle, he had considered the different sensibility of liquors, tho' this difference of sensibility be the only proof of the dilatation of glafs.

VII. *Observations on the declination of the needle, made in a voyage from France to the East-Indies, and in the return from the Indies to France, in the years 1703 and 1704, by M. Cassini the son **.

M. *Gualtieri*, the pope's nuncio in ordinary, communicated to us lately two charts, which were

* March 28, 1705.

given him by the chevalier *de Fontenay*, who commanded the vessels *le Maurepas*, and *le Pondichery*, which carried the pope's legate to the *Indies*. In these charts are described the course, they went day by day from *Port-Louis*, whence they sailed *April 23, 1703*, to *Malacca*, and from *Malacca* to *Port-Louis*, where they arrived in *August, 1704*. On one of these charts is described the variation of the needle, observed not only in the voyage from *France* to the *Indies*, but also in the return; and there is a much more considerable number of them, than *M. de May* has described on his chart, the relation of which we have already made to the academy.

Among these observations, there are many which were made on the same days with those of *M. de May*. There are also some in *M. de May's* chart, which are not described on the new chart; so that it appears they have been made by different observers, and perhaps even in two different ships, those which were made on the same day, not agreeing all of them exactly, having some difference in some of them, which nevertheless does not exceed more than half a degree. That agreement which we have already found between the variations observed by *M. de May*, and those remarked by *Dr. Halley*, in his chart, has induced us to examine those which we have since received; and we have placed on *Dr. Halley's* chart, all the observations which are remarked on these new courses, which are to the number of 94.

To place them with the greatest exactness possibly, regard has been had to the longitude, which is described in these different charts. In the chevalier *de Fontenay's* chart, the first meridian

ridian passes through *Cape-Verd*. The longitude of the *Cape of Good-Hope*, is there set down at 38° ; that of the isle of *Bourbon* or *Mascaregne*, where they landed going and coming, at 76° ; of *Pondichery*, $102^{\circ}, 30'$; and of *Malacca*, 122° .

In Dr. *Halley's* chart, which takes the longitudes from the meridian of *London*, the difference between the longitude of *Cape-Verd*, and the *Cape of Good-Hope*, is there said to be $33^{\circ} \frac{1}{2}$, which is $4^{\circ} \frac{1}{2}$ less than in the new chart. The difference which is between the *Cape of Good-Hope* and the isle of *Bourbon*, 38° ; between the isle of *Bourbon* and *Pondichery*, $25^{\circ}, 40'$; and between *Pondichery* and *Malacca*, 23° .

The difference of longitude between *Cape-Verd* and the *Cape of Good-Hope*, being not the same in these two charts, the necessary reduction has been made, to place on Dr. *Halley's* chart, those places where the declination was observed in the new chart, which are comprised between the meridians of these two capes. As to the other differences which are very near the same, there was no need of making any considerable reductions in them.

It is seen by comparing these observations, that there are many of them which give precisely the same declination of the needle, as is described in the chart of variations; and that the greatest part of them are not more than a degree distant from it. Some of them differ more considerably, principally those which were observed in the return, from $106^{\circ}, 50'$ longitude; and 5° south latitude, to $81^{\circ}, 30'$ longitude, and $20^{\circ}, 30'$ south latitude. These observations differ from those described in the chart of variations, from 3 to 8 degrees, and do not even

agree with those which were made in the voyage from *France* to the *Indies*, which are a little more southward. Thus it may be conjectured, that there was some particular cause, which produced these differences.

I have said in the preceding memoir, that if on examining the observations of the variation of the needle made in several other courses, there is found a like conformity with that which I have related, some use might also be made of it, in determining the longitudes, more especially in those seas which are beyond the equator, where the lines which mark the variations, cut the parallels more perpendicularly. This is confirmed by some observations, which *F. Noël* has communicated to us within these few days. This is what he relates.

“ Sailing in the *Indies*, in the year 1684, I
 “ perceived by several pilots journals, and by
 “ the conversations which I had with them, that
 “ the variation of the needle has certain bounds
 “ and fixed rules, at least in regard to certain
 “ places of the earth; so that when it is come
 “ to certain degrees N. E. or N. W. it returns
 “ towards the N. and never goes through the
 “ whole circle; so that formerly it was N. E.
 “ in some places, where it is now N. W. The
 “ annual difference of this variation, by comparing the journals of many years, was found
 “ to be from 0, 9', 30". In the year 1702, when
 “ I returned from *China* to the *Cape of Good-*
 “ *Hope*, the needle declined 12°, 30', from N.
 “ to W. At 100 leagues from this cape towards
 “ the *Indies*, it was 15°. At the point of the
 “ isle of *Madagascar*, it was 27°, which was
 “ much greater than when I first passed by there,
 “ going to the *Indies*. It keeps this rule pretty
 “ cer-

“ certainly, from the port of *Lisbon* to the *Indies*, so that pilots, by inspecting the needle’s variation, know certainly at what longitude from land, and at what place they are. It is now fixed in the middle of the passage, between the *Brasils* and *Africa*, that is, it neither declines to the E. nor W. It must be observed, that sometimes the needle loses its virtue by length of time, and through the bad temperature of the air.

VIII. *New observations on the load-stone and on the magnetical needle, by M. de la Hire, junior* *; translated by Mr. Chambers.

I do not here propose to give a new system of the load-stone, nor to enumerate, nor to relate the several virtues of that stone; or the effects observed in steel, or iron touched thereby, but shall only endeavour to clear up some difficulties which occur in observations made with a needle; and subjoin some remarks upon the nature of the load-stone itself, and its comparison with our globe of earth; which, by all the experiments hitherto made, appears to be a real load-stone.

’Tis known, that all observations of the variation of the needle made at sea, are liable to many errors, on account of the great quantity of iron in all vessels, which, according to its several positions, will draw the needle out of its true direction, not to mention the usual construction of this needle, or compass, as ’tis called, which is too gross to give the declination very exact; but the observations, which we now make at land with large needles, very delicately hung, as that of 8 inches long, which we have used from

* April 22. 1705.

the year 1682, after first determining a meridian plane with all the justness imaginable, and far from all ferruginous matter to apply the side of the box to have assured us of the just declination of the needle, and of its progression, or changing, called its variation, as may appear from several memoirs, which we have already published.

Some philosophers have imagined, not without a shew of reason, that the needles touched with different stones, do not exhibit different declinations, on account of the varieties observable in different needles at the same place, yet that such inequalities might arise from the different structure of the needles.

For needles touched with a stone, have only received from it a disposition in their pores, to let the magnetic matter, circulating in a certain direction round the earth, pass through them, after the same manner as the load-stones themselves received this property at their first formation. So that the different load-stones cannot give different virtues to the needles, which can receive no direction, but according to the course of the magnetic matter, which being the same in the same place of the earth, must give them the same direction with its own; but tho' the magnetic matter act equally, and in the same direction in the same place, it may be diverted therefrom, and this differently, according to the different disposition and figure of the bodies which receive it, as we find befall two load-stones, or two needles, when placed at liberty near each other out of the line of direction.

This made it be imagined, that needles, which have two pieces of steel at their ends joined together by a slender thread, might be considered as

two

two load-stones, of different strength and figure, placed apart from each other, but connected by some intermediate body ; and if the two pieces of steel were of such nature or figure, as that the magnetic matter should have a different direction in the one from what it has in the other, and that one of them had received a stronger impression from the touch than the other, it will necessarily follow, that the needle must take an intermediate direction compounded of the two, and different from that of the magnetic vortex of the earth ; consequently such needles must exhibit different declinations from each other, as well as from needles of a different make.

Needles made broad in the middle, when terminating in points, may be supposed less liable to such irregularities than those thus furnished with pieces of steel at their ends ; but yet they will not be entirely exempt therefrom, by reason of the inequalities of their matter and figure, which can never be perfect.

To ascertain this point, we procured 4 needles to be made bigger at the middle than towards the ends, and terminating in a very fine point, they were each of them 8 inches long, two of them being as strait and equable as possible, a third crooked like an S, and the fourth in shape of a bow ; one of the strait, and the two crooked ones, were touched with an excellent stone, which weighs 7 *lb.* and is strong enough to turn a needle 6 or 7 feet distance ; so that it must have a sensible *vortex* round it, above 12 feet in diameter ; the other needle was touched with a very good stone belonging to M. *Butterfield*.

We proceeded next to examine the box, which was of an oblong figure, in order to be satisfied whether its sides were parallel to each other, and

to the line which passes through the pivot, and the two first points of division of the two arches, which serve to measure the quantity of the declination, with regard to the point of the pivot; and the whole being thus rectified, we found by several observations, that the two strait needles, and that bent like an S, had their points and the bottom of their cap, which plays on the pivot, perfectly in a right line; as to the other arched one, we found one of its extremities deviate from the right line $2^{\circ}—20'$.

This done, we put the strait needle which had been touched with our stone, and which is the needle we commonly use to take the declination by in the box, and placing the side of the box against the meridional plane, the needle gave us $9^{\circ}, 25'$, declination westwards, which agrees with our other observations; then we put the other strait needle in the box, which had been touched with M. *Butterfield's* stone, and found it give exactly the same declination $9^{\circ}—25'$; yet another larger needle than either of them, which had two pieces of steel at the ends, and which had been touched with the same stone, had given us the declination $9^{\circ}—52'$ in the same place some time before; lastly, the needle bent like an S, only gave it $8^{\circ}—45'$; and the last, which was arched still less, viz. $8^{\circ}—22'$.

Hence we may conclude, that needles touched with different stones, do not give different declinations; and that if there be any difference, it must either arise from the unequal heterogeneous matter, or from the figure of the needle.

This is confirmed by the observations of the two strait needles; as to that shaped like an S, its two halves being placed aside of the right line which passes through its extremities, the point, which

which looked towards the north, only shewed $8^{\circ}—45'$ — in lieu of $9^{\circ}—25'$, which might proceed from the composition of the direction of the magnetic matter in the two parts of the needle, and perhaps also in some measure from the matter thereof. As to that shaped archwise, a right line passing thro' its two points, would have given us $9^{\circ}—32'$ — declination, which deviates but little from that given by the strait ones.

On the inequalities in the variation of the load-stone.

We shall not here relate the accounts of several authors touching the different declinations of the needle, the certainty of whose observations might be called in question; but only give those which we ourselves have made, and compare them with some others, whereof we may be fully satisfied.

M. *Picard* informs us in his treatise of the measure of the earth, that in the year 1670, he had found the variation at *Paris* $1^{\circ}—30'$ — westwards with a needle, 5 inches long. That in the year 1666, there was no sensible declination at all by the same needle; but that in 1664, it declined $40'$ — eastward, the change having been $20'$ — for each year. — In the manuscript of the same observations of M. *Picard*, we find in 1680 the declination of the same needle was $2^{\circ}—40'$ —; consequently from 1670 to 1680, the declination had only increased $1^{\circ}—10'$, which only gives $7'$ — for the yearly increase.

From that time we have made a greater number of observations at the royal observatory, with a needle 8 inches long already mentioned, I shall here only relate some of the principal ones.

In

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In 1683, *March* the 10th the declination was
 $3^{\circ} \text{---} 50'$ westwards.

In 1684 the needle declined $4^{\circ} \text{---} 10'$

In 1685 the declination still remained $4^{\circ} \text{---} 10'$

In 1686 it was $4^{\circ} \text{---} 30'$

In 1692 it declined $5^{\circ} \text{---} 50'$

In 1693 $\text{---} 6^{\circ} \text{---} 20'$

In 1696 $\text{---} 7^{\circ} \text{---} 8'$

In 1698 $\text{---} 7^{\circ} \text{---} 40'$

In 1700 $\text{---} 8^{\circ} \text{---} 12'$

In 1701 $\text{---} 8^{\circ} \text{---} 25'$

In 1704 $\text{---} 9^{\circ} \text{---} 20'$

By confidering these obfervations it appears, that the declination does not increafe equably, that fometimes it feems the fame for two years together, but afterwards increafes fafter than it fhould have done. Without entering therefore into the reafons of thofe little variations, we fhall content ourfelves to compare the diftant obfervations together, in order to deduce the law of the variation therefrom, there being no reafon to imagine, that from the time of its turning towards the weft, it has either increafed or slackened to the prefent time. Now fetting afide *M. Picard's* obfervation of 1680, we fhall find, that in 38 years, *viz.* from 1666, to 1704, the declination has increafed $9^{\circ}, 20'$, which only gives about $14' \frac{1}{3}$ for each year.

It appears alfo from fome ancient obfervations, that in the year 1580, the declination at *Paris* was $11, 30'$, eaftward, which being compared with the obfervation of 1666, when there was no declination at all, gives fomewhat lefs than $8'$ — *per year*; whence one would imagine, that the variation had not been fo great in thofe days
as

as now. Indeed, it may be observed by the way, that 'tis not very easy to measure and estimate minutes exactly, on a little circle of 8 inches diameter, and that the magnetic matter of the *vortex* of the earth might not be powerful enough to bring a larger needle exactly to the same point ; so that 'tis no wonder, if we sometimes find considerable distances between one year and another.

In a *Spanish* book, entitled the *Naval Theatre*, by *Don Francisco de Seylas* and *Louera*, we find two causes assigned for the variation of the declination ; one is the several mines of load-stones, found in the several parts of the earth ; and the other the nature of the load-stones, wherewith the needles are touched.——As to the former, there is no doubt but large rocks of load-stone may affect the needles when near them ; but that they should have an effect at a great distance can hardly be conceived.

As to the latter, the author builds on some experiments, which he made in a mine of load-stones in the province of *Honduras*, in *America*. This mine, he says, consisted of two principal veins ; one whereof was extended north and south, and the other east and west. In the former vein, he found a slip about two fingers broad, which was excellent load-stone, and placing a needle along this slip, he found it had no declination, but when the same needle was placed on the other vein, it had a sensible declination on both sides ; he adds, that he learnt hereby, that the north and south vein predominated over the other.

Now what he says is highly probable, but it does not follow hence, that when the stones are taken out of the mine, and needles touched with
them,

them, such needles must have the same direction, as the stone had in the mine; since in reality the needle does not take its direction from the stone, but only from the magnetic vortex of the earth; for otherwise, if the point of a needle were touched with that side of a stone, which looks to the east or west in a free situation, it would follow, that this point of the needle would turn to the east or west, which is contrary to all experience.

He adds farther, that having melted some of this vein of load-stone, he procured iron from it, which had the same virtue as the stone itself; and yet 'tis well known, that a load-stone, when heated only red-hot looses all its virtue, and much more must it do so, when it has undergone a fusion. What further discredits his whole account is, that he assures us, that putting two little pieces of this iron on the ends of a needle, it never varied either at land or sea, a circumstance by no means probable, and contradictory in some measure to his own relation. For what is now become of his rocks and mines of load-stone, which were laid down as the general cause of the needles varying.

Of the conversion of iron into load-stone.

If all the difference between load-stone and animated iron consist in this, that the former is of a stony nature, and capable of being broke and reduced into a fine dust; whereas the latter is incapable of both, by reason its parts are softer and more tenacious. 'Tis certain, that rusty iron, if it be possessed of a magnetic power, must be considered as real load-stone, since iron in this state seems to retain nothing of the metalline nature, but rather appears as a stone being easy to be broke and ground to dust.

Gassendus, in the life of *Fab. de Peiresk*, relates, that the cross on the steeple of *St. John d' Aix*, in *Provence*, having been thrown down by thunder, it was observed, that a crust of rust, which had gathered on the part of the iron of the cross inserted into the wall, was found very magnetical, though it had nothing remaining of the former quality of iron. This led some curious persons at *Chartres*, a few years ago, to examine whether the rust on the bars of iron that bound the stones of one of the steeples of *Notredame*, when they were obliged to rebuild it, were not also converted into load-stone, and after examining several pieces thereof, they found some which were really pure load-stones, having nothing of the iron remaining, while others had no sensible virtue; and others very little. Several of these magnets I have still by me.

On this occasion my father searched among a multitude of pieces of iron rust, some of them very thick, which had been gathered from antient buildings, but could not find one that appeared magnetical; this is easily discovered by presenting them gently to the point of an animated needle; for if they have acquired any power, they will attract this point on the one side, and repel it on the other.

Upon this, he bethought of a method of making this kind of load-stone with iron, not being able to attribute this conversion of iron into load-stone, but to two causes; the one the disposition of the iron in the air with regard to the magnetic vortex of the earth, which might have given it a magnetic power, which it still in some measure retained, tho' changed into rust or stone; the other to a particular property in some iron which disposes it to be converted into load-stone. —

To this purpose, he took one of the square stones of *St. Leu*, and sawing it to an angle of 60° — with the horizon, placed it in the air in the direction of the meridian line, making withal several grooves in the flat side, and inserting pieces of iron wiar into the same, according to the direction of the magnetic matter around the earth, with regard to our horizon. This was done in 1695, and the part of the stone, wherein the wiar were put, was covered over with the rest which had been cut off of these wiar. Some were animated, and others not; they were placed about the distance of two inches from each other.

The design of all this *apparatus*, was only to learn, whether when the iron wiar should in length of time be consumed, the rust arising from it would not be a magnetical matter, and whether there would be any difference between the iron which had been touched, and that which had not.—He chose the stone of *St. Leu*, as being that whereof the steeple of *Notredame* at *Chartres* had been built.

In fine, after the space of 10 years, we found that only some of the iron wiar were quite turned into rust, tho' none of them were above $\frac{1}{2}$ a line in diameter, but all the wiar, whether wholly or partly rusted, had a strong magnetic power, as appeared by presenting them to a needle; and those which had not been touched as much as the rest, which can only be supposed owing to the length of time which they had continued in a proper position to receive the effect of the magnetic vortex of the earth, and to their being converted either wholly or partly into stone. The pieces of wiar were 4 or 5 inches long, and were held horizontally when presented to the needle, to prevent their becoming animated by the vortex

tex of the earth. Upon the whole, such as were intirely changed into rust, were real load-stones; and the like might be said of those little flakes of rust, which came out of the rest; tho' as these flakes would not hang to the extremity of the iron-wiar, which was not magnetical, but hung vigorously to the point of an animated knife, one would imagine, that they were not changed into load-stone, but that they still retained something of the iron, unless we chuse to suppose, that such small particles of load-stone were not strong enough, with regard to their weight, to sustain themselves upon iron, which had not been animated.

It cannot indeed be absolutely said, that rust retains none of the properties of iron, since we have found that some large pieces of rust, which made no impression on a magnetical needle, supported by its pivot, when reduced to dust, would still hang to the point of an animated knife.

But these pieces of rust, which have no magnetic virtue of their own, are likewise incapable of receiving any from the touch of the best stone; it may therefore be imagined, that in this rust, which is about $\frac{1}{4}$ of an inch thick, and in all respects like good load-stone, the particles of iron still remaining, are too much fettered with the other matters mixed therewith, to be in a condition to receive the magnetic virtue of the earth's *vortex*; doubtless there is a good quantity of iron in the load-stones, which are real stones, since we can procure iron from them by the fire; but I hardly believe it possible to procure iron out of that which has been consumed by rust.

This experiment led us to make another; we took some of those little pieces of burnt and

melted iron, which fall in sparks and scales around the smith's anvil, and reduced them like a stone into a fine powder, which powder would stick very strongly to the point of an animated knife. But what was more, some of those pieces which had been melted, and were capable of being reduced to powder, were still susceptible of the magnetic power; and when touched with a good stone, took up a large quantity of steel-filings.

By this it appears, that the fire in melting and consuming iron, does not intirely destroy its nature, tho' it only leaves it in the appearance of a stone. There is little or no iron ore but is a load-stone, as may be easily known, by presenting it on several sides to a magnetic needle; but tho' they have the character of real load-stone, they are sometimes scarce able to sustain the smallest filings.

I have had a large load-stone for some years, which weighs a 100 *lb.* and tho' it be not of the best matter, it appears tolerably good by its effects, since it will turn a needle at $6\frac{1}{2}$ feet distance, and consequently must have a sphere around it of 13 feet diameter. This stone we have rounded in some measure, and filled up the greater inequalities with a plaister of the same colour as the stone, so that the whole makes a kind of ball, about a foot in diameter.

Having searched for the poles of this stone, we found them in two points diametrically opposite, upon which we drew an equator in the middle between them; and divided it from 30° , to 30° ; thro' which we drew meridians, in order to observe the several declinations of the needle with the more exactness; we also noted its declination in all the points where the meridians cut the

the equator ; by which it appears, that in a certain space it is western, in another eastern, and in several places 0 ; the greatest of these declinations is 26° , and three of the places which are no declination, are in the arctic circle. In effect, by tracing the several points wherein the needle had no declination, we formed two different lines, one whereof began from the arctic circle, and returned to it again, by a meridian, after descending about 10° beyond the equator, and describing a space about 110° , parallel to that circle ; the other beginning likewise in the pole or circle, makes several turnings near the same ; after which it takes its course north and south, and making some more inflexions, cuts the equator, and terminates in the antarctic circle. All which declination, and the lines where there are none, bear a near relation to what we find on the terrestrial globe.

By the several experiments hitherto related, it appears, that the different declinations of the load-stone, observed on the terrestrial globe, are only owing to the different disposition of the magnetic matter in the earth, as we judge, to be the case in our globular magnet, for there is no doubt but the vortex of magnetic matter, was the first cause of all load-stones, since we find it produce new ones every day ; and if this matter could have taken so many different directions in forming our stone in its mine, takes no fewer in the whole globe, and if changes could befall our load-stone like those which befall the earth, by the destruction of some magnetic matter, and the formation of other where there was none before, we should in course of time find variations on this load-stone, answerable to those which befall the magnetism of the earth.

*IX. On the condensation and dilatation of the air, by M. de la Hire, jun. * translated by Mr. Chambers.*

The general rule given by M. Mariotte, for finding the different condensations of the air, produced by given weights, is founded on an experiment which he relates in the first place, and afterwards confirms by 3 others, made with a crooked glass tube, one branch whereof having a foot, was sealed hermetically; the other being as wide as one pleased, he put mercury into this tube, and continued the experiment, as may be seen in page 140, &c. of his treatise of the motion of fluids; and the experiments arising herefrom, gave him occasion to establish a general rule, and to advance, that the density of the air is in proportion to the weight.

My father also laid before the academy a general rule for the condensation and dilatation of the air, deduced from this only supposition, that the air is heavy and elastic.

He made several experiments, to find in what proportion a spring taken in a moderate state of extension, would be stretched by different weights, and found, that its extensions were in a direct ratio of the weights; but proceeding also to enquire how the spring contracted, he found, that its condensations were not in a direct, but a reciprocal ratio of the same weights, which will not be difficult to conceive, if we remember, that in stretching, the springs enlarge their bulk, in proportion as the weights are augmented; and that on the contrary, in contracting they lessen

* May 6, 1705.

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the same. It was on these experiments, therefore, that he built his general rule, which proves perfectly conformable to that of M. Mariotte, and to the experiments lately made before the academy by M. Amontons, as will appear by the following table, where his experiments, and the result of the rule, are found opposite to each other.

Height of mercury in the tube.	Density of air by the experiment.	Density of air by the rule.
Inches.	Parts.	Parts.
6	9 $\frac{6}{9}$	9 $\frac{7}{9} + \frac{103}{133}$
12	8 $\frac{2}{9}$	8 $\frac{4}{9} + \frac{62}{133}$
14	7 $\frac{7}{9}$	7 $\frac{8}{9} + \frac{131}{133}$
18	7 $\frac{1}{9}$	7 $\frac{2}{9} + \frac{107}{133}$
24	6 $\frac{2}{9} \frac{1}{2}$	6 $\frac{1}{9} + \frac{180}{133}$
30	5 $\frac{8}{9}$	5 $\frac{5}{9} + \frac{207}{133}$
36	5 $\frac{1}{9} \frac{1}{2}$	5 $\frac{2}{9} + \frac{231}{133}$
42	4 $\frac{5}{9}$	4 $\frac{6}{9} + \frac{270}{133}$
48	4 $\frac{2}{9}$	4 $\frac{3}{9} + \frac{383}{133}$

X. Experiments on the rarefaction of air, by M. Amontons *, translated by Mr. Chambers.

I filled a tube 40 inches long with mercury, and found it to contain 7 ounces, 7 drachms, 8 grains.

I filled in like manner another tube, one end whereof terminated in a large vesicle, of the figure of a cervelas, and found it hold 87 ounces, 6 drachms. The vesicle in particular, to its setting on to the tube, contained as much as a tube equally thick, with that of 46 inches, and 475 inches 5 lines $\frac{5}{8}$ long; the rest of the tube, which

* June 10, 1705.

was

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was 29 inches long, contained as much as 36 inches 6 lines and $\frac{1}{2}$, of the same tube of 46 inches, so that the whole tube with its vesicle, represented a tube 511 inches, 8 lines $\frac{3}{4}$, long, and equal in thickness to that of 46 inches.

This tube with its vesicle, being full of mercury, I inverted it as usual; excepting that to prevent heating the vesicle and its contents, I always handled it with a linnen cloth, which was observed in all the following experiments.

The lower end being immersed an inch deep in mercury, which rose above the edge of the bason, in proportion as the vesicle emptied itself, the mercury at length stopped in the tube 28 inches above the mercury in the bason, which shewed, that the atmosphere was then equal to such 28 inches.

While the vessel was emptying, I observed abundance of large air bubbles all along the tube, attempting continually to rise, but still prevented by the descent of the mercury, but at length they ascended and reached the vesicle, when the mercury ceased falling; this air I took to be what the mercury was purging off.

To see whether this air did not alter the height of the mercury, I repeated the experiment with the tube of 46 inches, and the mercury stopped there likewise 28 inches above the mercury in the bason.

Having assured myself of the weight of the atmosphere, I filled the tube with the vesicle again, and poured a little mercury out into the tube of 46 inches, to see what height it would there possess. By this means I found that the air left in the tube was equivalent to 2 inches 6 lines of the tube of 46 inches, and the like of all the rest; whether after filling the tube intirely, I measured the

the mercury poured out, or whether without filling it, I measured what was put in it by subtracting it from the total capacity of the tube.

The natural bulk therefore being two inches, 6 lines, I inverted the tube upon which the mercury stopped 2 lines lower than the 28 inches; that is, 27 inches, 10 lines above the mercury in the basin, and consequently these 2 inches, 6 lines were spread thro' a space above 190 times greater than what they possessed before, yet still preserved a spring equal to two lines.

Having left 18 inches, 7 lines of air, the mercury after inverting the tube remained one inch, 1 line below the 28 inches, which shall henceforward be the term, from whence I compute the falling of the mercury.

Having left 36 inches, 6 lines $\frac{1}{2}$ of air, the mercury stood 2 inches, 1 line $\frac{1}{3}$ lower.

Leaving 46 inches, 8 lines $\frac{2}{3}$ of air, that is, putting only the 46 inches tube full of mercury, it stood 25 inches, 9 lines $\frac{1}{4}$ lower.

Putting in the tube of 46 inches twice full of mercury, it stood 23 inches, 9 lines lower.

Putting in thrice the same tube full of mercury 21 inches, 1 line lower.

Putting in four times the same tube full of mercury, it stood 18 inches, 7 lines $\frac{1}{2}$ lower.

This way of measuring with the tube of 46 inches appearing too tedious to be continued, I had recourse to weighing.

Putting therefore 2 lb. 7 ounces, 3 drachms, 40 grains of mercury, which is 5 times the quantity of what fills the 46 inch tube, the mercury remained 16 inches, 1 line $\frac{1}{4}$ lower.

Putting in 3 lb. 7 ounces, 1 drachm, 56 grains of mercury, which is seven times the quantity, it remained 10 inches, 11 lines lower.

Putting in 3 pounds, 15 ounces, 64 grains of mercury, which is 8 times the quantity, it remained 7 inches, 11 lines lower.

Putting in 4 *lb.* 7 ounces of mercury, which is 9 times the quantity, it remained 5 inches, 7 lines $\frac{1}{2}$ lower.

Putting in 4 *lb.* 14 ounces, 7 drachms, 8 grains of mercury, which is 10 times the quantity, it remained 3 inches lower.

Putting in 5 pound, 6 ounces, 6 drachms, 16 grains of mercury, it sunk 4 lines lower.

It must be observed, that supposing all the measures and weights above-mentioned exact, there must be some 5 inches, 8 lines $\frac{2}{7}$, of the vesicle tube wanting of being full, which should be the natural bulk of the air in this experiment, which, however, was only found to be 5 inches, 6 lines $\frac{1}{2}$; so that there was an error of 1 line $\frac{1}{2}$ or about a line $\frac{1}{2}$, which is not very considerable in a length of above 511 inches, being only the $\frac{1}{600}$ part thereof, which I only mention to shew, that there was no very gross error in the measures and weights, and to signify, that we are to take the natural bulk of the air in this experiment, to be 5 inches, 6 lines $\frac{1}{2}$, in lieu of 5 inches, 8 lines $\frac{2}{7}$, which with 11 times 46 inches, make the whole length 511 inches, 8 lines $\frac{2}{7}$.

These experiments made, I filled the vesicle tube intirely again, and inverting it, the mercury stopped as before at 28 inches.

Now to find whether these experiments agree with the received hypothesis, a table may be made, wherein the product of the natural bulk, by the weight of the atmosphere, may be expressed in one column, and the product of the bulk dilated by its respective load in the other.

My opinion was, that all the terms, which give these products, were only determined by the measures in the experiment, somewhat near the truth, and not to the utmost exactness, and therefore that I could not suppose any of them true, more than others, nor of consequence infer any thing with certainty from them; and the rather as each of these measures, beside its particular variation from the real bulk, might likewise vary from the hypothesis by an error in the other 3 measures.

Thus, for instance, if the measure of the bulk dilated be less than the real magnitude of this bulk, the experiment will seem to deviate from the hypothesis by the particular error of such measure, that is, by making this dilated bulk smaller than the calculus; tho' it must be confessed, if there were no other error to be apprehended, this would not deserve much notice, and the rather as it is the common practice.

But if besides the dilated bulk being taken for less than it is, the natural bulk be taken for greater than it really is, this second error, after inverting the tube, will add a new magnitude to the dilated bulk of the calculus, and render the difference between the calculus and the experiment still more considerable.

And again, if the weight of the atmosphere be taken for less than it is, as the same weight occasions a greater change in a quantity of air much dilated, than in the same quantity of air when less dilated, the calculus will on this account give the bulk of dilated air greater than the experiment.

Lastly, if in measuring the tube, its dimensions be taken greater than they really are, this will likewise cause the bulk of the dilated air to be augmented by the calculus.

On account of these four errors, in respect of measures, which are no errors in the hypothesis, I apprehended, that the bulk dilated, as found by the calculus, might differ considerably from that of the experiment, without any thing being deducible therefrom against the truth of the hypothesis. On the contrary, this seemed to make it impossible to distinguish whence the difference between the calculus and the experiment might arise, unless the experiment deviated very far from the hypothesis; for in this case, the conclusion must be against the hypothesis, since the measures can only deviate a little from the truth, and consequently are incapable of producing any considerable difference.

I apprehended therefore that so long as the difference between the calculus and the experiment should not be very considerable, it would be almost impossible to say, whether it proceeded from an error in the measures, which, by the nature of the thing, are all thrown together, or from the erroneousness of the hypothesis.

But notwithstanding all this, some very knowing members of the academy, to whose judgment I owe all imaginable deference, having reckoned, that the quantities of the atmosphere of the natural bulk of the air, and the length of the tube, may be taken for absolutely true; I shall no longer stand out, but am even inclined to suppose with them, that these magnitudes are true.

On this footing the difference between the product of the natural bulk of the air by the atmosphere, and the product of the bulk when dilated by its load, will be the difference which we are to suppose between the hypothesis and the experiment; though if my sentiment had obtained all that

that could have been inferred therefrom is, that as these products are nearly equal, we have a strong inducement to believe, that the hypothesis and the experiment do not vary from each other.

XI. *On the vernal froths, or cuckow-spittle, by M. Poupart * ; translated by Mr. Chambers.*

In the spring season, we find a sort of white froth, sticking indifferently to all kinds of plants. We denominate it vernal from the time of its appearance, which is always in the spring, either sooner or later, as the season is more or less early.

Several naturalists have treated of this froth, without understanding its cause. Some will have it to be vapours, which, exhaling from certain grounds by the warmth of the spring, gather upon such plants as they meet withal. — The reason they alledge, is, that we sometimes find a little spot of ground where there is scarce a plant without it; tho' perhaps nothing of the kind shall be found for 20 or 30 miles around; which argues that all lands are not proper to produce such froths.

Isidore Hispalensis takes them to be the cuckow's spittle; an opinion which he probably entertained from their form, and the time of their appearing and disappearing, which correspond pretty nearly to those of the cuckow: add, that in its flight from one place to another, this bird makes a kind of rattling in its throat, as if it were going to spit.

Others imagine it to be the extravasated juice

* June 20, 1705.

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of plants; and *Moufet* will have it no other
than a frothy dew.

Swammerdam of all the naturalists has best entered into the reason of these froths. His doctrine is, that the grasshoppers yield them, at their mouth: and 'tis true, they are the produce of those animals, tho' not at the part he mentions. So that he only speaks by conjecture.

I could relate several other opinions as to the nature of these froths; but being all of them false, I shall dwell no longer upon them. The real case is as follows: In the summer time, we find a diminutive kind of grasshoppers, called by some naturalists *flea-grasshoppers*, from their jumping like fleas. Their hind legs do not reach above their backs, as those of all other grasshoppers do, but are always folded under the belly, like those of fleas. This enables them to jump very fast, by reason there is no time lost between one jump and another.

I have already observed, in the *Journal des Savans*, for *August* 1693, that this kind of grasshopper has a stiff-pointed needle, or trunk, whereby it sucks the juices of plants. — This observation suggests something new, and curious; in as much as this is the only species of grasshoppers that has a trunk: all the rest, hitherto known, have a mouth, lips, and teeth wherewith they devour herbs, and even the vine.

Vos locustæ —————

Ne meas ledatis vites: sunt enim teneræ.

These flea-grasshoppers lay eggs; from whence, in the ensuing spring, arise young ones, which remain for some time inclosed in a fine membrane. This membrane is no other than a mask,
with

with eyes, feet, wings and other organs, which are the cases of the like parts of the little animal inclosed therein. When it issues from its egg, it appears like a little whitish worm, no bigger than the point of a needle. Some days after it turns green, by means, as we may suppose, of the juices of the plants it is fed withal. In this state it resembles a kind of little green frog, which creeps up trees, and for that reason is called *rana arborea*, or the tree-frog. The insect, tho' wrapt up in its membrane, will yet walk boldly, and at a good rate; but does not leap, or fly till it have put off its skin.

As soon as it leaves its egg, it mounts upon a plant; and clapping its *anus* thereto, deposits a little drop of white liquor, full of air. Close to this, it drops a second, then a third; and continues after this rate, till it have involved or buried itself in a thick froth, from whence it never stirs, till it be delivered from its membrane, and have attained its state of perfection.

To cast forth this froth, it makes a kind of arch with half its body, the belly being the convexity thereof. This done, it begins another arch, opposite to the first, whereof the belly, which was the convex, becomes the concave part; each time it makes this double compression, a little froth or bubble springs from its *anus*, which it afterwards enlarges, by pushing it this way and that with its feet.

I placed several of these little grasshoppers upon a young mint, and found that those leaves they cast their froth upon, did not grow or advance in the least, tho' those opposite to them grew to their natural size. This shews, that the insect lives of the juice of the plant, while inclosed in the froth.

The

The young grasshopper being by means hereof arrived at a certain bulk, strips off its coat, or cover, which it leaves in the froth, and leaps about in the field.

The froth, we suppose, may have served to defend it from the heat of the sun, which would otherwise dry and wither it up; it also preserves them from the spiders, which would suck them up, as I have sometimes found.

The country people hold these froths a prognostick of fine weather. But all the matter is, that they are only found when the weather is fine; ill weather spoiling and destroying them.

XII. *Philosophical experiments upon the refraction of musquet-balls in water, and upon the resistance of this fluid, by M. Carre *, translated by Mr. Chambers.*

There being several persons who doubt whether musket-balls undergo any refraction, that is, any change in the direction of their motion when shot obliquely into the water, and I having asserted it, as a known fact, in the history of the academy for the year 1702, upon the authority of several former writers, I desired a friend, who has been some time at his country-house, to examine into the truth of it; and the following experiments are the result of this request.

First, I shot a musket-shot loaden with a ball once, and a second time into a stone basin full of water, 2 feet $\frac{1}{2}$ in diameter, and 16 inches deep; the first time under an angle of 20 degrees, and the second under an angle of 80; but I could not perceive whether the balls underwent any change of direction, by reason of the great effort of

* July 11, 1705.

the water against the sides of the bason, which always displaced the piece of past-board, which had lodged there for a mark. This effort is so violent, that upon shooting three times into tubs of water, they were all instantly burst; and in particular it was always the lowest hoops that the water made give way. To satisfy myself that it was really the great motion and effort of the water that burst the vessels, and not the ball by passing thro', I provided a square chest about a foot high, and $\frac{1}{2}$ a foot thick, the 4 boards whereof, which formed the length, were each an inch thick, and the two at the end, each 2 inches thick, that the others might be well fastened thereto, with store of nails, then filling it with water by a little hole, I exploded my piece, upon which the ball perforated the boards very exactly without bursting them; but the water was shook so violently, that it tore the boards from each other, and burst the chest.

Secondly, To make a more precise experiment on the refraction of a ball in water, I filled a stone bason therewith, whose length on the inside was 3 feet, 3 inches, its breadth 1 foot, 8 inches, and depth 1 foot, 1 inch, here represented by ABCD *. To the side BD, I fastened a board to receive the balls, another board EF precisely in the middle, and another CD upon the bottom to cover it intirely; the boards were all strongly fastened, that the strokes of the ball might not shake them. Over the side CA, I fitted a piece of pastboard GA perpendicular to the horizon, and the musquet HN was fixed 8 feet from the bason, upon exploding it the ball pierced the pastboard in K, and was found flattened like a shilling at M. I shot a second time,

* Plate V. Fig. 5.

and found the ball broke into 3 pieces, which were likewise flatted, without appearing to have struck against the board EF. I shot two balls with a stronger charge of powder; but they were neither of them to be found, either at the bottom of the bason, or in the boards. These balls were a 3d of an inch in diameter, and being made on purpose for the piece, filled it very exactly.

Thirdly, To try anew the experiments of refraction, I bethought myself of a way to get an insight into this flattening of the balls; to this end, I procured a reservoir 10 foot square, and fitted therein two boards parallel to the horizon, and to each other, and about a foot apart, the upper one making the same plane with the surface of the water, then shooting two balls upon this board under an angle of 30 degrees, and with an equal charge of powder; the first, with the piece used before, whose barrel was 3 feet, 2 inches $\frac{1}{2}$ long, and the bullet 3 lines $\frac{3}{4}$ diameter; the second, with another piece, whose barrel was 3 feet, 10 inches, 3 lines, and the ball 7 lines in diameter. Here the large ball pierced the two boards, and consequently traversed the whole extent of water between them; whereas the small one only pierced the upper board, and was found quite flatted on the lower.

Fourthly, But to satisfy myself whether there be really a refraction, I made use of the stone-bason, described above, and prepared after the same manner; then fastening my piece upon two rests, one of them 5, and the other 7 feet distant from the bason, I nailed it down, so as to make it remain in the same situation after the explosion, as before. It now made an angle of 20 degrees with the horizon, or the surface of the

water in the bason, and was charged with 3 penny weights, 20 grains of powder, and a ball 7 lines in diameter, weighing 17 penny weights, 6 grains, shooting it off, the ball, after piercing the past-board in K, and the board EF in P stuck in the point R, where I found it fast. - Upon this, emptying the bason, I drew a thread from the middle of the ball in R, thro' the holes P and K, to the middle of the mouth of the piece, and found that the thread passed exactly enough thro' the centres of all the holes.

I repeated the same experiment, by setting the piece a little on one side, that the ball might pass thro' a fresh place. The effect was, that piercing the past-board $\frac{1}{4}$ an inch from K, and the board EF $\frac{1}{2}$ an inch from P, it fixed likewise $\frac{1}{4}$ an inch from R; so that the centres of the two balls wanted not a line of being in the same parallelism to the horizon; hence it may be inferred, that if there be a refraction, it is not sensible.

Being willing to try whether the ball of 7 lines diameter would be flattened by increasing the charge of the piece, I used 7 penny weights 6 grains of powder, and found accordingly the ball about M flattened on one side; it had struck the board EF slightly; but this could not have occasioned its flattening, since a ball will pierce 3 or 4 boards without losing much of its sphericity. Putting the same charge of powder in the former piece, I found the ball about M, cloven into two parts, each of them flatted unequally, tho' without having ever touched the board EF. Shooting a second time with half the charge of powder, the ball did not reach the board EF, nor lost any thing considerable of its sphericity.

Fifthly, To procure further satisfaction as to the flattening of the balls, I spread a linnen cloth parallelly

to the horizon, under two feet depth of water, in a basin 40 feet in diameter, and 6 feet deep, then charging my piece very high with powder, and loading it with a ball of 7 lines diameter, I discharged it against the linnen, and found the ball flattened thereon, tho' very unequally, and of an irregular figure; then charging the piece anew with $\frac{1}{3}$ more powder, the ball divided itself into several little pieces, whereof I found five upon the linnen, most of them about the bigness of a lentile, but differently shaped.

I then shot another ball perpendicularly to the surface of the water, which was found flattened very regularly, being almost as round and flat as a shilling; whereas all those which had been shot obliquely had flattened irregularly.

It may be proper to observe, that in shooting a ball into the water, it rises or rears itself above the same more or less, as the charge of powder is greater or less, and I have even seen it rise 20 feet high.

It will not be very difficult to assign the cause of these several effects, with regard to which I must observe, that 4 penny weight of powder drive the large ball of 7 lines a considerable space in the water, without taking any thing from its sphericity; that with 8 penny weight of powder, it loses $\frac{1}{2}$ its sphericity; and with 12, the whole; and lastly, with 16, breaks into several parts.

Reflections upon the experiments above.

There are two things to be considered in these experiments, which I can warrant to have been made with great care and exactness: the first, that the ball shot in an angle of 20 degrees changed nothing, at least as to sense of the direction of their motion. The second, that these
balls

balls when exploded with a great charge of powder, turned flat as they entered the water.

First, It should seem at 1st sight agreeably to the common notion of the resistance of fluids, that a ball shot into the water under an angle of 20 degrees, or even a greater angle, should not only change its direction but even rebound; for as the quantity of matter in all bodies is proportionable to their weight, and the resistances of fluids are in the same *ratio* as their densities, and the density or weight of water is to that of air, as 800 or 850 to 1, according to some experiments, or as 1175 to 1, according to those of the academy *Del Cimento*; the water must make 800 times more resistance than air, and consequently diminish the velocity of the ball by the same quantity. Now 'tis easy to shew pursuantly to the reasoning of M. *Descartes*, and several others, that if the ball, upon meeting the water loose half its velocity, it must rebound. — For suppose a circle * EGD, whose diameter EF represents the separation of the surfaces of air and water, and suppose the angle ACE to be 20 degrees, whose sine will be AB, and CB the sine of its complement, the ball now being driven from A to C, we may consider its motion as composed of the vertical one AB, and the horizontal one BC, as CB therefore is much greater than half of CE. If the ball lost half its velocity upon meeting the water, it should of consequence rebound; but experience shews us, that it does not.

It must therefore be made appear, that the ball loses so little of its vertical motion, that it not only should not rebound, but should not even lose any thing sensible of its former direction. —

* Fig. 6.

In order hereto, we are to observe, that 'tis when the ball enters the water that its direction is to be changed, and that its motion in AC, being composed of a horizontal motion, equal to BC, and a perpendicular one, equal to AB, we are only to consider this latter, since the former makes no opposition thereto; and again, that this ball finds no more resistance on one side than on the other, till such time as half its magnitude is immersed under water; for which reason, if the bulk of water, whose place it possesses, were equal in weight to half the ball, as would be the case, if the ball were shot into melted lead, 'tis evident the ball would lose near half its velocity; but it being only water that opposes its motion, and the weight of lead being to that of water, as 12 to 1, if in meeting with the bulk of water equal to its own, it must only lose $\frac{1}{2}$ part of its motion, since, as already observed, the resistances of fluid bodies to their being separated are in the *ratio* of their densities, or weights: but as here we have only half to consider, the ball must only lose $\frac{1}{4}$ part of its vertical motion; that is, a 24th part of AB, which being but a small matter, the change of direction must necessarily be insensible. If it be alledged, that this 24th part should be considerable enough to be perceived, by reason it must be referred to this, that the ball has two boards and a past-board to pierce, which may occasion some little alteration in its motion, perhaps the motion of gravity of the ball might likewise be considered, which has a tendency to carry it directly downwards, and consequently to prevent its change of determination from being sensible; but the great velocity of the ball, and the little space it passes, must render this next to nothing. 'Tis otherwise how-
ever

ever with the rays of light, by reason of their passage from air to water, or other transparent mediums. There is no proper local motion, but only certain inequalities of pressure, or resistance, in the different mediums, as has been explained elsewhere; so that this does not hinder us from concluding, that the rays of light always pass more easily into the densest mediums, and that 'tis on this account they approach towards the perpendicular.

It must be observed however, that if a ball be shot very obliquely, so that the angle of incidence only contains a few degrees, its horizontal motion being now very great with regard to the vertical one, it may meet such a multitude of particles of water at the same time, as may prevent its entering, and oblige it to rebound; and such is the case, when we make what the boys call ducks and drakes.

The second thing to be considered in these experiments, is the flattening of the balls upon meeting the water; and at first sight, it may seem surprising, that a fluid body, such as water, which yields and divides so readily, should make a resistance like a solid body; but if the great velocity of the ball be considered, 'twill appear possible for it to meet such a quantity of particles of water at the same time, as that their resistance shall be equivalent to that of a hard body, and turn it flat. In drawing the hand with some velocity thro' the water, we find a certain resistance; and if it be drawn with twice the velocity, the resistance will be fourfold, since we have twice the number of particles to move in the same time with a double velocity; and if the same hand be drawn with thrice the swiftness, the resistance will be 9 times greater; so that the resistances

resistances increase in the *ratio* of the squares of the velocities. Hence the velocity of the hand may be so great, as that the particles of water it meets in any given time may afford it a resistance equal to that of a hard body ; in effect, if the hand be stretched parallelly to the surface of the water, and struck with nimbleness against this surface, the resistance will be so great as to be painful, and I remember, that beating the water strongly one day with a stick, it broke in my hand.

From hence 'tis easy to account for the flattening of musquet-balls shot into water, and what confirms the theory is, that the higher the piece is charged, the more flattened are the balls found ; for that having a greater velocity, they meet more particles of water in the same time. — And hence we may account why an end of a candle being loaded in a fowling-piece will pierce a thick plank of wood.

XIII. *A comparison between our observations on the barometer, and those made by F. Sebastien Truchet, by M. Maraldi *.*

Among the observations on the barometer, which F. *Sebastien* last related to the academy, we have chiefly considered those which he made at *Clermont*, and on the summit of *Mount d'Or*, the highest mountain in *Auvergne* ; the perpendicular height of which, we determined above the surface of the sea by the angles, of the meridian, but could not make the barometrical experiment upon it, it being at that time covered with snow.

* July 5, 1705.

This

This year 1705, *June* 8, at 4 in the afternoon, *F. Sebastien* observed on the summit of *Mount d'Or*, that the quicksilver in the barometer was suspended at 22 inches, 2 lines.

The same day at noon, we found the height of quicksilver at the observatory 27 inches, 9 lines $\frac{1}{4}$, and at 7^h 24' in the evening, it was 27 inches, 9 lines $\frac{1}{4}$, having encreased but $\frac{1}{4}$ of a line since noon.

Between the 22 inches, 2 lines height of quicksilver observed at *Mount d'Or*, and the 27 inches, 9 lines $\frac{1}{4}$, observed at *Paris*, the difference is 5 inches, 7 lines $\frac{1}{4}$, which the quicksilver at the observatory kept higher than at the top of *Mount d'Or*. We have found by our experiments, that the quicksilver on the sea shore is commonly 4 lines $\frac{1}{6}$ higher than at the observatory. The quicksilver was therefore 5 inches, 11 lines $\frac{2}{3}$ lower on the mountain, than it would have been at the same time on the sea-shore.

In the memoirs of the academy of 1703, we said the perpendicular height of *Mount d'Or* above the surface of the sea was 1030 toises : but *M. Cassini* the son, by a more exact calculation, found it afterwards to be 14 toises higher ; so that its perpendicular height above the surface of the sea will be 1047 toises, to which we have found as above, that a variation answers of 5 inches, 11 lines $\frac{2}{3}$ in the height of the quicksilver.

By the progression founded on the experiments related in the memoirs of the academy for the year 1703, the quicksilver should, to this height of *Mount d'Or*, sink 5 inches, 7 lines, which is 4 lines less than by the observation.

M. de la Hire has communicated to us the observations which he made the same days on the barometer : and tho' his barometer and ours

are on the same floor of the observatory, yet these observations sometimes differ a little; whether it proceeds from their having been made at different hours of the day, or from any other cause.

By *M. de la Hire's* observation *June 8*, at $\frac{1}{2}$ after 5 in the morning, the height of the barometer was 27 inches, 8 lines $\frac{1}{3}$. If this observation is compared with *F. Sebastien's*, in the same manner as we did ours, there will be found between the level of the sea and *Mount d'Or*, a variation of 5 inches, 10 lines $\frac{1}{2}$ in the height of the barometer, which agrees within 3 lines with what is given by the progression.

F. Sebastien made another observation with the barometer at *Clermont*, near the convent of the *Minims*, which is that part of the city where *M. Perier*, in the year 1641, made his experiment on the barometer, the same day whereon he carried it on *Puy de Domme* to find the variation of the barometer, which answers to these two heights.

June 10th, at 6 in the evening, *F. Sebastien* found the height of the quicksilver at *Clermont*, to be 26 inches, 6 lines. At *Paris*, by observations made before and after, it was 27 inches, 10 lines high.

Therefore the difference of the quicksilver between *Clermont* and *Paris* was 1 inch, 4 lines.

We have said above, that between *Paris* and the level of the sea, there is in the height of the quicksilver a variation of 4 inches $\frac{1}{6}$.

Between *Clermont* therefore and the surface of the sea, there must then have been 1 inch, 8 lines $\frac{1}{6}$.

By *M. Perier's* observations made in 1641 between the convent of the *Minims*, and the top of *Puy*

Puy de Domme, there was a variation of 3 inches, 1 line $\frac{1}{2}$ in the height of the quicksilver.

From the level of the sea to the summit of *Puy de Domme*, there must, according to these different observations, be in the height of the quicksilver a variation of 4 inches, 9 lines $\frac{2}{3}$, which answers to 810 toises perpendicular height, from the summit of *Puy de Domme* to the level of the sea. By the progression settled in the memoirs, 55 lines $\frac{1}{4}$ variation of quicksilver agrees with this height, which is but two lines different from the observation.

But by *M. de la Hire's* observation made on the same day *June 10*, the barometer was 27 inches, 8 lines high; and making the same reductions and comparisons, as in the preceding observation, there will be between the sea shore, and the summit of *Puy de Domme*, a variation of 55 $\frac{1}{3}$ in the height of the quicksilver, as taken from the progression settled in the memoirs.

XIV. *Remarks on some experiments made with several barometers, and on the light made by one of them, which was agitated vertically, by M. de la Hire the son **.

We have two barometers in the observatory; one of which has a tube of 1 line $\frac{1}{2}$ inside diameter, and is 36 inches $\frac{1}{2}$ in height; and consequently when the quicksilver is at 28 inches, there remains 8 $\frac{1}{2}$ void. It is this which we commonly make use of. The other is 2 lines inside diameter, and 32 inches $\frac{1}{4}$ in height, and consequently there is but 4 inches $\frac{1}{4}$ remaining above the 28 inches. It was this barometer *M. Picard*

* Aug. 12, 1705.

made use of, and which was the first wherein a light was observed, by agitating it vertically. It still continues to give as usual a very great one: the other gave none, though filled with the same quicksilver; this is most certain, for it was very often looked upon. Yet within these few days having been agitated, it was seen to give almost as much light as the other.

This barometer of M. *Picard's* was emptied and filled many times without any precaution to cleanse the quicksilver and the tube; yet it always gave the same light as at first. But we observed, that though this light was very lively, so as to be seen at candle-light and moon-light, by exposing the tube to it: yet if in the day-time a room was shut exactly close, so that it was quite dark, and the barometer a little time after was therein agitated, no light thereof was to be seen, which made us at first imagine, that it gave no light in the day-time: but endeavouring to be more certain of this experiment, we continued in the room, where it gave no light for above a quarter of an hour; and then agitating the barometer, which during that time we had put in the sun, we saw its light as great as in the night. This last experiment destroyed the imagination we had conceived, and gave us to understand, that the retina requires a considerable time to lose that vibration, which is caused in it by the light of the sun.

The height of the quicksilver in these two barometers differs always 3 lines $\frac{1}{2}$, which M. *Picard's* is higher. We have lately made another. The quicksilver was strained through a fine clean linnen, and the tube, which was of 3 lines diameter, and 35 inches long, was well cleansed with spirit of wine, and afterwards thoroughly

wiped with very dry linnen drawn through it ; and after having very carefully filled it, that no visible bubble of air might be left therein, we observed, that the quicksilver kept at 1 line $\frac{2}{3}$ lower than in M. *Picard's* barometer, and very near the same quantity higher than in the other.

But in trying this barometer, we observed that after having filled the tube with quicksilver, and evacuated all the air, and dipped the open end of it when stopped with a finger into the quicksilver, the tube being at first very much inclined ; when it was raised, and the void began to appear above, some almost imperceptible little bubbles of air were discerned, which suddenly became as big as little pease, and entered into the *vacuum* ; some of them being confined between the quicksilver and the tube, and others seeming to come out of the quicksilver, and performing the same effect as if it was boiling. We also observed, that these bubbles which came out of the quicksilver by raising the tube, when they became a little big, disappeared by lowering of it, and seemed to re-enter into the body of the quicksilver ; for on that part where they disappeared, nothing was to be seen against the tube. This barometer when agitated gave no light.

There is no question but that all these bubbles, as well those confined between the quicksilver and the tube, as those which seemed to come out of the quicksilver, were little particles of air enclosed and confined therein, which being then disburthened from all the weight of the atmosphere, and from the height of the quicksilver, which compressed them in the tube when the open end was above, occupy a very great bulk, with respect to that which they before occupied ;

cupied ; and it is certain, that the more slender and the longer the tube is above 28 inches, the more of these bubbles will escape into that space which the quicksilver quits ; all the quicksilver which occupied that place having there purged itself from the air. Therefore it will appear, that tubes must be taken of a length proportioned to the places where the barometers are to be put, and they are to be left but one inch above the greatest height of the quicksilver in the place they are in, and that they be about 3 lines in diameter, rather more than less, and the quicksilver well purged from the air. With these precautions, I think, barometers may be made as exact as possible.

We now make no longer doubt but that the barometer, which we commonly make use of, whose tube is slender and too long above the 28 inches, has many of these particles of air confined in the quicksilver, and between the quicksilver and the glass, which being disengaged when it is tried, have occupied a considerable place in the top of the tube, and that this is the real cause why the quicksilver therein is lower than in those which are larger and shorter, wherein the bubbles, when dilated, have not so sensible an effect : from the reasons above-mentioned,

It must be observed, that those circumstances which were thought necessary to make barometers luminous, seem, by what we have now said, to be destroyed.

XV. *On the height of the quicksilver in the barometers, by M. Amontons †.*

This experiment is of great consequence; for it lays us under the necessity of examining over again all the observations made on the barometer to this day.

It has hitherto been thought, that the height of the quicksilver in barometers was always visibly the same in the same place, and it was far from being imagined, that, with glasses very near alike filled with the same care, and the same quicksilver, the heights of this quicksilver should differ from one another in the same place, and at the same time, 18 lines, or thereabouts. Yet this is what I shall make appear to the company, after having observed that one principal reason, which may have hindered this *phænomenon* from being perceived, is, that most of those who construct barometers, have unfortunately neglected to place thereon such graduations, as truly express the heights of the quicksilver, and have almost always in the room of these substituted arbitrary graduations, which bear no relation to the heights of the quicksilver; occasioned, without doubt, from a true sense of the difficulty there is in making these sorts of instruments uniform, and that it would enhance the price, and lessen the sale of them. Thus interest is frequently an obstacle to the discovery of truth.

It appears, therefore, that it was not without great reason, that in my barometers I rejected these kind of arbitrary graduations, being fully persuaded, that it is impossible to make a profitable use of barometers by exact observations, if

† Aug. 14, 1705.

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the degrees are not divided into parts, which express the inches and lines of the height of the quicksilver with which they are filled; and also, if they are not regulated by the same barometer, which should be as a standard and rule for them, without which all must be uncertain, and lead to error.

Seeking the reason of the *phenomenon* which I relate, it is difficult to avoid attributing it to the inequality of the pores of different glasses, which give more or less passage to the particles of air, according as they are more or less open; which seemed the more likely to me, being certain, that the glasses of the two tubes, with which I made this experiment, are different in quality.

We owe this discovery to the lord chancellor. He has a simple barometer mounted in the *English* manner, that is, one of those which has two little copper-plates, on which are marked the different dispositions which may happen in the air, as fair, changeable, rain, &c.

My lord chancellor had, for a considerable time, experienced with satisfaction the indications of his barometer; but at last it being out of order, he applied to M. *Homburg*, who mended it for him. Since when the variations of this barometer have always been in the lower parts of the copper-plates, that is, in those parts which indicated nothing but rain, wind, and storms. My lord chancellor observing nothing like this in the disposition of the air, sent for me to examine his barometer. The first thing I did, was to see by inclining it, whether the *vacuum* was well made; and having found it as well as possible, and that the quicksilver had all the freedom of motion which could be desired, I answered my lord, that I saw nothing could hinder its having
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ing effect. He then explained to me what he had observed in the manner I have mentioned ; and I begged leave of him, that I might carry his barometer home with me to examine it at more leisure, which he granted. I measured, as soon as possible, the height of the quicksilver, and finding it only 26 inches, 6 lines, while 3 other glasses which were in use, and in which the *vacuum* was not even so perfect as in that, were at 28 inches, I at first thought this might proceed from the quicksilver, which had perhaps a more than ordinary weight ; which made me immediately take the barometer to pieces, and having filled one of my tubes with this very quicksilver, it stopped at 28 inches, as in the 3 others which were in use. I then filled the glass of the barometer with other quicksilver, but it always stopped at 26 inches, 6 lines ; which gave me then no room to doubt, and I knew that this effect could only proceed from the glass. I then changed this glass, and put another into the barometer : and in this new glass the quicksilver kept up 18 lines higher than in that which I had taken away ; so that as the barometer played before in the lower part of the copper-plates, it would now have played in the upper parts, had I not raised the plates about 4 or 5 lines ; and my lord still thinks, they ought to be raised higher : whence it may be supposed, that the glass which I took away, was not that which was at first there, in which the *vacuum* was probably made of a mean height of what is observed in these.

XVI. *A continuation of the observations on the height of the quicksilver in the barometers, by M. Amontons *.*

Inspecting the glass of my lord chancellor's barometer, having guessed that it was bought of *sieur Deville* enameller, I went to find him when I left the academy; having asked him, he said it was so. I caused him to make me four others; that is, two of the same glass, and two of another sort of glass; when I had filled all of them with quicksilver, and also those two which I had made use of in the experiment at the academy, the quicksilver stopped at different heights in them all.

The greatest height was 28 inches.

The second, half a line less. This was that glass of the academy wherein the quicksilver remained highest.

The third, one line $\frac{1}{2}$ less.

The fourth, seven lines less.

The fifth, seven lines $\frac{1}{2}$ less.

The sixth, ten lines less. This was the glass wherein the quicksilver at the academy remained lowest.

So that the difference in the second height, which, in the morning, I had found to be 18 lines, and in the afternoon, at the academy more than 19 lines, was at $\frac{1}{2}$ an hour after 8 in the evening but 9 lines.

I left all these glasses in trial, and the next day found them of the same heights. But this great difference of 18 lines, which I now found to be only 9 lines puzzled me. As nothing to my knowledge had happened to the quicksilver, ex-

* Aug. 19. 1705.

cept that it had been much handled, I judged, that perhaps the dirt and moisture of the hands might partly have stopped the pores of this glass. I therefore emptied out the quicksilver to wash the outside, and scour it as well as I could with spirit of wine: but after having done this, and filled the glass again with its own quicksilver, I found this difference still diminished 1 line $\frac{1}{2}$, which made me resolve to meddle no more with it. I have left it ever since, and it has only varied like all others, that is, it has fallen about 2 or 3 lines.

As all this is very unaccountable; in order to endeavour to give some light into a thing wherein there is so little, with deference to the opinion of the company, mine would be to choose among a number of glasses, those which being filled with quicksilver give heights of sensible difference from one another, and apply them all to the same graduation; or, which is the same thing, on the same vertical plane, at the bottom of which should be a kind of common trough, full of quicksilver, in which they must all dip. Above this trough, beginning at the surface of the quicksilver, should be 29 or 30 parallel lines drawn from inch to inch: the 4 or 5 last should be subdivided from line to line by other parallels.

It would be also convenient to add to all these glasses another of equal length, but uniform from one end to the other, sealed hermetically at each end, in which should be about 28 inches of quicksilver; and in the empty space thick air.

This tube would serve to show the effect of heat on the quicksilver, and whenever the quicksilver in the other glasses has no other motion but what is equal to this, there should be no regard

had to them, as not being an effect of the weight of the atmosphere. Such a tube, by rights, should for the future accompany all simple barometers.

This whole machine constructed in this manner, ought to be carefully observed for a considerable time, and thereby a certainty might be gained.

1st, Whether the variations happen in all the glasses at the same time.

2dly, Whether they are equal in all of them, or whether they are not rather proportionable to the heights of the quicksilver, with which each glass is filled; which seems very likely, if it is true, that the pores of the glass give way to the parts of air, which are small enough for that.

XVII. *A continuation of the observations on the height of the quicksilver in the barometers, by M. Amontons †.*

M. Homberg having informed us, that he had washed the tube of the lord chancellor's barometer with spirit of wine, it occasioned some to suspect this might be the cause why the quicksilver in this tube remained lower than in the others; which seemed to me so much the more probable, as I recollected, that when I first examined this barometer, the little reflection of light, which the bending at the top of the quicksilver commonly makes, appeared to me more obscure than usual; this being occasioned, as I now imagine, from a little of the spirit of wine remaining in the tube.

† Aug. 22, 1705.

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That which hindered me, from perceiving it, was,

1st, That the quicksilver seemed to me very clear through the whole length of the glass, without any little bubbles of air such as are usually formed when the tube is not very dry.

2dly, Because having, as I have already said, held the barometer sloping, I found the *vacuum* as well made, as in those glasses which are filled in the best manner.

And also this great difference which I at first found in the heights of the quicksilver of this barometer, from that of my other glasses, and which diminished by degrees, as I emptied and filled again this tube, seemed to me a confirmation of the fact, it being impossible this effect should be any other than a consequence of the dissipation of that little spirit of wine.

In short, to be convinced and satisfy what had been resolved, I washed the inside of this tube with spirit of wine, rubbing it pretty hard with a little cotton fastened to the end of a brass wire; then letting it drain a whole night (which seemed to me long enough, considering how easily spirit of wine is known to evaporate) I filled it with quicksilver, together with the other tube, in which the quicksilver had always kept very high, which I did not clean, though it appeared very foul. After which I really found there was the 19 lines height of quicksilver difference between these two glasses, which I had found at the academy, and the little edge of the quicksilver obscured.

Though hereby the fact seems to be sufficiently cleared; yet the difficulty of explaining the cause thereof subsists. For in short, it no ways appears, that this spirit of wine should be reduced
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into air, as might be imagined; for this air must have had the strength of a spring, equal to 19 lines of quicksilver, and the glass being put into a horizontal situation, this air would also have occupied therein near 5 lines, whereas nothing of this is even now to be seen; yet the tube makes an angle of about 45 degrees with the horizon, or thereabouts.

Besides the new tubes, wherein the quicksilver kept 6 or 7 lines lower in some than in others, and whose difference diminishes also by degrees as they are emptied and filled again with the quicksilver, without any reason to suspect that they ever had any spirit of wine in them, gives room to imagine that the spirit of wine occasions a less height in the quicksilver, only by making the glass cleaner, and preventing the quicksilver from fouling the inside of the tube, which perhaps stops part of the pores of the glass. But why in barometers, which have been long mounted, this filth does not continue to stop intirely these pores, is what cannot easily be accounted for?

It is true, that this obstruction of the pores of the glass, seems to be made only in proportion, as these glasses are emptied and filled again with quicksilver; and perhaps one and the same glass has not often enough been emptied and filled again for this to be perceived.

However it is, it still appears difficult to explain the phænomenon in question, except by supposing that there passes a greater quantity of the least particles of air through those glasses, whose pores are most open and less incumbered, as I have already said in my first observations, and that perhaps more considerable differences would be found, if tubes were used which were made of a matter different from glass.

XVIII. *Experiments on the capillary tubes,*
by M. Carré *; translated by Mr. Cham-
bers.

Most modern authors, who speak of the weight and spring of the air, take notice of the experiments made with capillary tubes, and endeavour to explain why water rises therein far above its level; and the farther as the diameter of the capillary tube is smaller, the opinions are very much divided hereon: Some will have this ascent owing to the pressure of the atmosphere, being greater on the surrounding water, than within the tube; others account for it from the air in the tube, not being at liberty to move and act with all the force of its spring against the water rising therein; others hold, that it is occasioned by the water's moistening the inside of the tube adhering thereto, and being in some measure sustained thereby; so that the lateral columns of the water, which surrounds the tube, having more force, that is, relative weight, than the columns of water in the tube oblige them to ascend.——But in physical matters, it being experiment alone that is to decide. I have been induced, on account of the great number of celebrated authors, which have spoken hereof, to make several experiments on this head, and the generality of them in consort with M. Geoffroy.

1st, We took 3 capillary tubes, the largest whereof was $\frac{1}{3}$ of a line in diameter; the 2d, $\frac{1}{6}$ of a line, and the smallest, $\frac{1}{10}$. These we plunged in water, and to wet them the better, passed some of it quite through them; then putting them in a vertical situation, the water rose

* Aug. 22, 1705.

above its level in the 1st ; 10 lines in the 2d ; an inch $\frac{1}{2}$, and 2 inches $\frac{1}{2}$ in the smallest.

Then taking the 3 tubes, we stopped one of their ends with a piece of wax, and fastening them one after another to the scale of a very nice balance, letting the open end dip in a vessel of water underneath, and having disposed them thus, we put them in perfect equilibrium. The piece of wax at the upper orifice, was intended to prevent the water from entering them that way, and was removed when the tubes were balanced and laid in the scale with the tube, to prevent any alteration in the equilibrium ; immediately whereupon the water rose in the tubes, to the heights above specified. Now I argued before the experiments, that if the water rose in the tubes by an unequal pressure of the air, the equilibrium would remain the same after as before ; but if the ascent were owing to the water's moistening and sticking to the sides of the tubes, the water thus adhering, must be considered as a little weight superadded to the tube, and consequently the equilibrium would be destroyed.

The event was, that the water, as it rose in the little tube, made no alteration in the equilibrium, but that the equilibrium was broke, as it rose up the middle tube, and still more so in the large tube where the scale with the tube invisibly descended.

— Hence it should seem, after the reasoning above-mentioned, that the cause why the water ascended in the tubes, was its sticking to their insides, and that the question was decided ; but reflecting, that when one of the ends is closed with wax, the tube and the air within it, may be conceived as one single body, whose bulk is lighter than that of the water, whose place it possesses, and consequently must remain in the same

same equilibrium ; but that the tube being afterwards unstopped, and the air thus set at liberty to come forth, and the water to enter, we are henceforth only to consider the proper matter of the tube, whose bulk is heavier than an equal bulk of water ; so that this cause alone must destroy the equilibrium, and therefore nothing can be determined from these experiments, as to the real cause why water ascends in capillary tubes.

Taking the largest tube, and immersing it first in spirit of wine, the liquor rose therein about 3 lines $\frac{1}{4}$ above its level, and plunging it a second time, it rose 4 lines. — Then immersing the same tube in common water, it rose 5 lines $\frac{1}{4}$; and immersing it a second time, 7 lines $\frac{1}{4}$; the third time, it rose 10 lines. — Immersing it next in oil of turpentine, the liquor rose 4 lines above its level ; then immersing the same tube thus moistened with oil of turpentine, after first passing spirit of wine through it to cleanse it in spirit of wine, that liquor did not rise so high as the level of what was in the vessel, the reason whereof was apparently owing to a little drop of the liquor adhering to the inside of the tube. — The tube hereupon was immersed in oil of tartar per deliquium, which rose upwards of 5 lines above the level ; and being immersed a second time, it rose 6 lines. — It was immersed in spirit of nitre, which rose 4 lines. — Then in oil of olives, which rose 5 lines, the tubes where-withal these experiments were made being 12 inches $\frac{1}{2}$ long.

Another was then taken of the same diameter 9 inches $\frac{1}{2}$ long, which being immersed in common water, the fluid rose, as in the former, about 10 lines above its level ; immersing it then in spirit of wine, it rose 4 lines. — Whence it ap-

pears, that the different length of the tube makes no difference in the ascent of the fluid.

Immerging this tube in mercury, it rested below the level, and immersing one of the smallest size, the mercury had no visible rise.

Immerging another tube, 15 inches long $\frac{1}{6}$ of a line in diameter, the spirit of wine rose therein about 12 lines. — Immerging it in common water, it rose 2 inches, 5 lines.

Another tube, 5 inches long, and of the same diameter as the former, being immersed in spirit of wine, the liquor rose likewise near 12 lines; and when immersed in common water, rose 2 inches 3 lines $\frac{1}{4}$. — A little piece of a capillary tube being immersed in water, it rose to the top thereof, and there stood.

It appears from all these experiments, that common water always rises the highest, tho' we see nothing deducible herefrom, which may give any light into the difficulty in question; for as spirituous liquors are lighter than water, if their ascent above the level arose from the unequal pressure of the air, those liquors should rise higher than water; the contrary of which is found; add that as they are more subtle, they should moisten the parietes of the tubes more readily, and consequently adhere more plentifully, which would also occasion them to rise higher.

3dly, I immersed a capillary tube in a vessel of water, which rose therein 3 or 4 inches above its level. In this state I suspended the capillary tube, and put the whole under the receiver of an air-pump; reasoning thus, if it be the unequal pressure of the air that occasions the water to ascend in this capillary tube, when the air is exhausted from the receiver, the water must descend and return to a level with that around it; but

but if adhesion be the cause, no alteration must ensue.—The event however proved contrary to this reasoning; for after the air was exhausted, the water was so far from descending in the tube, that it rose upwards of a line higher. The reason whereof may not be far to seek; for water being full of a multitude of particles of air, when its spring is no longer bent by the pressure of the upper air, it must necessarily dilate, and augment the bulk of water. To be the better assured of this augmentation of bulk, I put the capillary tube in another tube, $\frac{1}{4}$ an inch in diameter, which I had filled with water, and marked the height thereof with ink, then exhausting the air, the water visibly rose above its mark; whence it may be inferred, that there are particles enough of air in water to make it susceptible of some condensation.

4thly, But the following experiments determine the question, and seem to leave beyond all doubt, that 'tis by the near adhesion of fluids to the insides of tubes, that makes them rise above their level; so that the other causes alledged by several authors have no share therein.—I run a drop of tallow into a capillary tube melting it to such a degree, till the layer of tallow was extremely thin upon the inside of the tube, for fear of stopping it up, then immersing it in water, the fluid only rose in the tube to a level, with the other water around it.

This single experiment shews, that the unequal pressure of the air is a fiction; for indeed how can such inequality be conceived, when the aperture of the tube is so great with regard to pores, through which the air is able to insinuate with a great deal of ease, and produce the same effects, as if it were at liberty, which is

easily proved; 1st, from the experiment of the common barometer, one of whose ends has been stopped with a hog's bladder; for after making the *vacuum* as usual, so that the pressure of the adjacent air keeps the mercury suspended 27 or 28 inches high, if a little hole be made in the bladder, with the point of a needle, whose diameter is much smaller than that of a capillary tube, the air will immediately insinuate into the tube, and make the mercury fall. 2dly, From what befel me one day in making experiments upon mercury, when the mercury continued falling after the *vacuum* was made, and upon enquiry into the cause, I found a little crack in the tube made use of, passing therefore two slips of parchment as exactly thereon as I could, I repeated the experiment, but the mercury still descended, tho' somewhat slower indeed, a sufficient proof of the extreme subtilty of air which can insinuate into the smallest apertures, and communicate its action thereby.

What confirms the adhesion of water to the inside of the tube is, that if we only run tallow into a part of the tube below the natural level of the water, the fluid in this case will rise above its level; and if the tallow be only run on one side of the tube, the water on that side will stand at its level, but will rise on the other side above the level. In fine, if a drop of water be let fall along the outside of the tube, this drop, upon arriving at the extremity thereof, instead of falling down, will enter within the tube, but if the tube be smeared with tallow, instead of entering it will fall directly down.——'Tis evident therefore from the whole, that the cause of the water's ascending above its level in capillary tubes is, that in wetting the parietes of the tube, it adheres to them,

them, and is partly sustained thereby, by which means the other columns of the adjacent water having a greater degree of weight, or pressing more on the bottom of the vessel, oblige those over the aperture of the tube to rise above their pitch.

To conceive how the external columns of water should have more weight than those immediately contiguous to the insides of the tubes, we advance the following proposition. ——— If a body rest by one of its extremes on the inequalities of some other vertical body, whether by meer opposition, or by entering within such inequalities, and be sustained by a power applied to the opposite part, the power will be to the weight, or to the effort it makes to descend, as the distance of the centre of weight of such body from the point of support, is to the distance of the power from the same point of support.

For suppose * AB, a vertical surface, and suppose a body as ED, one of whose extremes rests, or is sustained on the point D of the same surface, and having the point C for its centre of gravity, 'tis evident, that if a power sustained it in the point F, it will not bear the whole weight thereof, since we suppose it sustained in D; but this power will have the same *ratio* to the effort the body FD makes to descend, as the distance DC has to the distance FD; for we may suppose this body as sustained on the middle of a horizontal lever FD, by two powers applied in F and D, now by the laws of equilibrium the power F is to the weight of the body FD, as CD is to FD; wherefore, &c.

'Tis easy to apply this reasoning to capillary tubes; for suppose the vessel AB full of water, wherein the capillary tube CD is immersed, and suppose this

* Plate V. Fig. 7.

water divided into columns, composed of globules of water, laid one over another as E, 'tis evident, that when the water is entered this tube, all the particles in immediate contact with its parietes, will be partly sustained thereby; now by the laws of fluids, the water will stand at a level, provided nothing hinder it, by reason all its columns are equally heavy, or press the bottom of the vessel with equal force; but those which touch the inside of the tube are partly sustained, and consequently do not act on the bottom of the vessel with all their force; so that the adjacent columns must make them ascend, and this till such time as they make up in height, what they lose of their weight by adhesion, and thus restore the equilibrium.

From this account, it appears, that fluids, contiguous to the outsides of tubes, should likewise rise to a considerable height, which is contrary to experience; but it must be noted, that in the insides of tubes, the particles of the fluids sustain one another, and contribute to their mutual elevation, which does not obtain on the outside; accordingly we find that water will rise a little, even in a very large tube.

'Tis evident, that the smaller the diameter of a capillary tube is, the higher must the water rise therein; for the force of adhesion is measured by the internal surface of the tube, and the resistance is measured by the weight of the columns of water contained therein; but columns of the same height are in a duplicate ratio of the diameter of the tube; whereas the surfaces are only in the simple ratio of those diameters, consequently the surface of a large tube bears a less proportion to the quantity of water it contains, than the surface of a small one does to its contents of water,
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consequently the force of adhesion is less in the large than the small one; wherefore, &c.

'Tis evident likewise, that in tubes equally, or unequally, inclined, the water must always rise to the same height, notwithstanding its being in greater quantity than when are they vertical; for in inclined tubes, the *momentum* of the pressure of the water is not measured by the whole length of the tube, or by the absolute weight of the whole column of water in the tube, but by its vertical height, since it will only be impelled by the weight of the column of the adjacent water that presses freely.

I shall here subjoin some other experiments on the same subject, which tend to confirm this reasoning. — Suppose a capillary tube * AB, whose inside is very dry, applying only the end B to the surface of a stagnant water, it will rise to C; but if the tube be first wetted by passing water thro' it, it will rise higher to D; and if the tube be plunged in the water, it will rise still higher as to E; if now the tube be taken out of the water, that within it will gradually descend, and a little drop will be formed in B, which is the case when the height BE is very great, for otherwise the water will remain suspended, and not issue out; if now the water in B be made to touch a drop of water placed on a plane, the water in the tube will be found to descend from E to D, which is the place where it would have stood, if only the end B had been made to touch the surface of the water. On the contrary, if the water had only rose to C, and the end B had been made to touch the same drop of water on the plain, the water would have rose to D.

* Fig. 8.

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The cause of all these effects depends on the same laws of equilibrium; for when the drop in B touches another, it unites thereto by immediate contact, and in this case, if the water be in E, being now raised too high, it must sink, since the whole must come to an equilibrium; and if it be too low, as in C, it must rise for the same reason.

We are now to explain, why some bodies are easier to wet than others; why different fluids are fitted to wet different bodies; and, lastly, why certain fluids mixed together, while others will not mix, but always separate from each other.

In order to this, I lay it down as a certain principal, proved in my opinion beyond all possibility of contradiction, by father *Mallebranch* and M. *Bernoulli*, 1st, That the cohesion and hardness of bodies arises only from the compression of a surrounding fluid; for without admitting a kind of *gluten* in the parts of homogeneous bodies, a name no more intelligible, no fitter to explain the union of certain bodies than of sympathy, pretending to unite the particles to each other, true philosophy will allow no other force nor action in bodies, but what arises from their motion. 2dly, That this cohesion or hardness is so much the greater, as the particles of such bodies touch in more surface, and leave less fluid between them, to resist the action of that without them; so that if the resistance be equal to this external action or compression, the particles will not cohere at all; or if the internal fluid resist more than the external one, the particles will fly asunder; but if the external one act with most force, the same particles will unite, and this in a greater degree, as their surfaces are smoother

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in the several parts where they join; so that if they be so smooth, as to fit immediately to each other, without leaving any interstices between them, and consequently any passage for the ambient fluid, they will be compressed by the whole force of this fluid; and herein consists the greatest hardness of bodies. After this manner, we may account for the union of two smooth bodies, as 2 pieces of glass, 2 polished marbles, &c. or the union of 2 hollow hemispheres of brass, from whence the air has been exhausted, which will make such an effort against their being dis-united, that several horses will be required to separate them.

The application of this to fluids, which will wet some bodies and not others, is easy; for when the particles of a fluid have their texture or constitution of their little surface, such as that they may join most immediately to the surfaces of bodies, they are contiguous withal, and leave the least fluid between them and the surface of such body, they will, in this case, adhere, and be, as it were, fastened and kept together by the pressure of the surrounding fluid; and 'tis for this reason, that the drops of water sustained upon the leaves of trees, some whereof are extremely smooth, do not fall. And from the same principal we may explain, why the particles of any fluid unite, and those of a different fluid keep asunder; for the particles of one and the same fluid being all homogeneous, that is, having their surfaces nearly similar to each other, when they chance to meet they unite as close as possible, leaving very little of the above-mentioned fluid between them, to resist the action of the external one. On the contrary, the particles of different liquors being heterogeneous, that is,

having different figures, must of consequence leave a large quantity of the fluid between them, to prevent their joining. Hence, after mixing oil and water, by beating them together a considerable time, as the particles of fluids have each of them their motion in all possible directions, separately from the rest, when a particle of oil happens to meet a particle of water, they cannot join close enough to cohere, by reason of their figure, and the arrangement of their particles, which makes them to slip over one another, instead of closing; but when a particle of oil meets another particle of oil, their surfaces being similar, they approach as near as possible, and thus unite by reason of the little resistance made from within against the action of the fluid without.

Let it not be alledged, that this account tends to destroy the fluidity of liquors; for tho' a particle adhere close enough to another to be raised, or sustained therewith, on account of the smallness of its weight, yet it does not adhere strongly enough to resist the impulse of any other particle striking against it, or even the action of the subtil fluid, which may insinuate between them; by pursuing this reasoning, we may be able to account for the following very curious experiment. — Wine and oil being mixed together as intimately as possible, if we would separate them again, we are only to take two slips of whited brown paper, used for filtration withal, and dip them separately, the one in wine, and the other in oil, then immersing one end of each in the mixed liquor, and letting the other hang over the edge of the vessel, we shall find the oil drawn off by the paper, which had been moistened therewith, and the wine by the other. Now the reason is evident, for a particle of wine when it strikes

strikes against a particle of oil, being unable by reason of its figure, to come close enough to expel the subtil fluid between them, instead of uniting thereto, is necessarily repelled; whereas a particle of oil, on the contrary, meeting a particle of wine, approaches thereto closely enough to expel the fluid, upon which the surrounding fluid compressing them without resistance, they remain united together, and arise as usual.

When a greater number of fluids is mixed together, their separation is not effected with so much exactness; but it appears by the experiment, that water disengages itself the most readily from the other liquors it is mixed withal, which may serve to account for the great facility wherewith urine separates from the blood, in passing thro' the glands of the kidneys, as we shall shew hereafter.

The same principle might likewise perhaps serve, to explain the different secretions made in the body, that is, how the different particles whereof the blood consists, are separated through the glands of the respective viscera, destin'd for this office; for all the other solutions hereof labour under great difficulties: there are two, among many, which have the most air of probability. — The first is, that all the particles of blood are homogeneous, but that the pores of different glands being different, these serve as so many moulds, to give that figure to the particles received within them necessary to constitute the liquor required. Now 'tis hard to conceive how the chyle, which must consist of particles of all the different kinds of foods should be so changed, as that such different particles should all become homogeneous in the blood; besides how shall we conceive these moulds to act upon liquors which still remain fluid?

The second account supposes particles of matter in the blood of all kinds of figures, but that the pores of the glands being likewise differently figured, answerably thereto, let only such particles pass, as are adapted to them; that is, if a pore be prismatical, or pyramidal, it will only admit such particles as are of those figures. — This opinion would carry the more probability, if the particles of blood were of equal bigness; but as some of them are certainly bigger than others, we do not see why a cubic particle, for instance, which is smaller than a prismatic pore, should not pass thro' the same, and so of others; but if the glands be supposed to have been moistened, from the first formation of the body, with the proper liquor, which they are afterwards to secrete (as may be supposed consistently enough with the modern system of generation, which is, that all organized bodies were formed at the time of the creation, and lodged one in another, to be enlarged and unfolded in process of time) it will be easy to explain, upon the principle already laid down, how the heterogeneous particles of the blood should separate from each other, and compose the several juices wherewith we find the receptacles of the body filled; for a particle of bile, for instance, striking against a particle, which is to compose some other humour, will not adhere thereto, by reason of the different texture of their surface; but, for the contrary reason, will unite to another particle of bile, and both of them go together, to the common receptacle of the bile. After the same manner may we account for the nutrition and growth of different plants in the same soil, on a supposition, that there are particles of all figures in such soil; some of them fit for the nutriment of one plant, and some of another.

XIX. *A continuation of the observations on the height of the quicksilver in the barometers, by M. Amontons **.

Following my first views, I mean supposing that the pores in some tubes are more open than in others, and that admitting a passage to more parts of air, it is refused to none but the thickest, which by their weight support the remaining quicksilver in the tube ; I took a mean barrel of a gun, of 34 inches $\frac{1}{3}$ in length, I had the breech of it soldered, which is properly sealing it hermetically. When it was cold, I filled the barrel quite full with quicksilver, of which there entered 53 ounces $\frac{1}{2}$. I observed, that it held it exactly, not letting any out at any part ; after which I prepared to perform the inversion, but this tube not being transparent, the difficulty was to know at what height the quicksilver would stop. I at first thought to have weighed that which should remain in the tube after the inversion, that comparing it afterwards by the weight of quicksilver, which quite filled this barrel, I might judge of that height which I sought. But this not only seemed to me troublesome to execute, but I also, for many reasons, thought it not very certain. For, 1st, I was not sure that this barrel was exactly of the same size from one end to another : on the contrary, there was some reason to believe it was not ; therefore nothing precise could be gained this way. 2dly, Stopping the open end with a finger, in order to hinder the quicksilver in the cup from communicating with that in the tube, it was in a manner impossible, but that the quicksilver must

* Sept. 2. 1705.

then

then be in such balancings, as might give greater or less heights than what are true. After having considered all this, I succeeded in the following manner.

I caused the wooden vessel ABCDE * to be turned, the cavity of which was in the figure of a rectangular cone inverted, and the outside was a cylinder.

Then having taken the quicksilver out of the barrel, I laid the open end of it in the bottom of the wooden cone, and holding it sloping as much as possible, I poured a little quicksilver all round it, to see at what height I should make the hole FG, which might serve to discharge the quicksilver of the vessel ABC, and leave only therein always exactly a sufficient quantity to hinder the external air from entering at the bottom of the barrel.

After having therefore bored the hole FG, a little sloping towards E, I stopped it with a little wooden stopper, which I could take out and put in as I pleased; then I filled my iron tube quite full of quicksilver, thrusting into it an iron rod, which I turned about every way, long enough to expel all the little bubbles of air, which might remain adhered to the inward walls of this tube.

Having then poured into the vessel ABC a quantity of quicksilver, sufficient for the open end of the tube to dip therein, I put this vessel into a larger, in order to receive that which might run over by the discharge FG during the experiment.

After having dipped the open end of the tube full of quicksilver into that of the vessel, instead of raising it perpendicularly, as is usually done, I held it very sloping, so that in all appearance,

* Plate V. Fig. 9.

the *vacuum* must not be made in the upper part.

The whole being in this state, I opened the hole G to let out all the superfluous quicksilver, which came out immediately ; after which I, by degrees, set the tube upright, observing exactly the time when I saw the quicksilver run again, through the hole G ; for that was to show me the point where the *vacuum* was to be begun, which having several times carefully executed, holding a rule graduated by inches perpendicular to the side of the tube, I always found the perpendicular height of the quicksilver above F, at 23 inches, 4 lines, though it was then in other glass tubes at 27 inches, 8 lines.

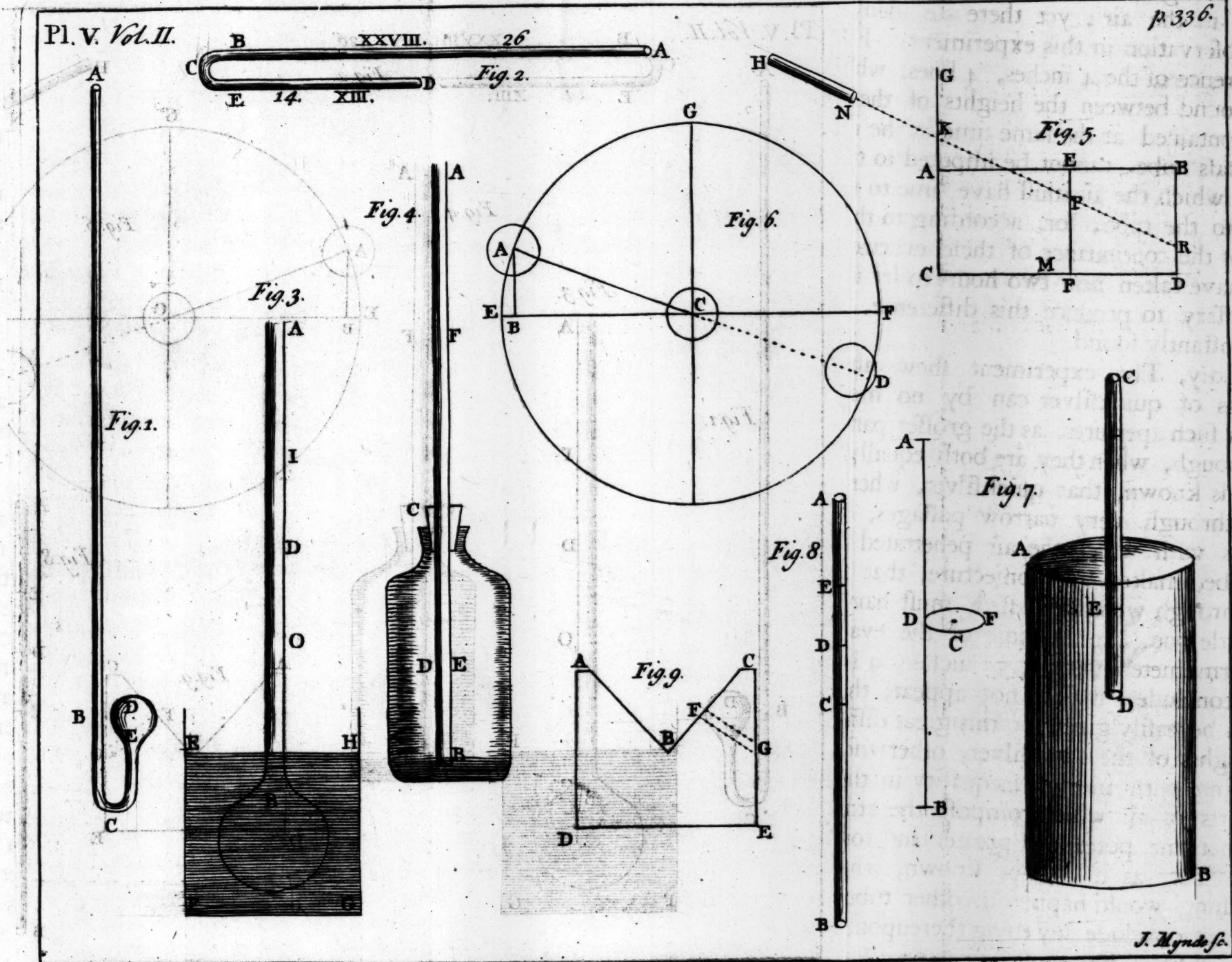
I then left this tube for the experiment : but during the first five hours there came out about 13 ounces $\frac{1}{2}$ weight of quicksilver. In the six hours following, there came out 6 ounces $\frac{1}{4}$ more ; and 10 ounces in twelve other hours ; and, lastly, 8 ounces in eight hours more : after which, having quite emptied the tube, I found still therein 4 ounces $\frac{3}{4}$; so that all the quicksilver which remained in the tube, after the inversion, was 43 ounces, which was to the 53 $\frac{1}{2}$, which filled the tube near the *ratio* of the 27 inches, 8 lines, which the glass tube had given, to 34 inches $\frac{1}{3}$ length of the iron tube : which would have made me imagine, if I had regard only to the weight of quicksilver, that the *vacuum* was made of the same height in the iron as in the glass tube. But it is to be observed, that the iron tube during the evacuation, was so sloping, that the quicksilver must stand therein about 6 lines higher, than if it had been perpendicular, and that also the size of the iron tube in all appearance lessened near the top ;
which

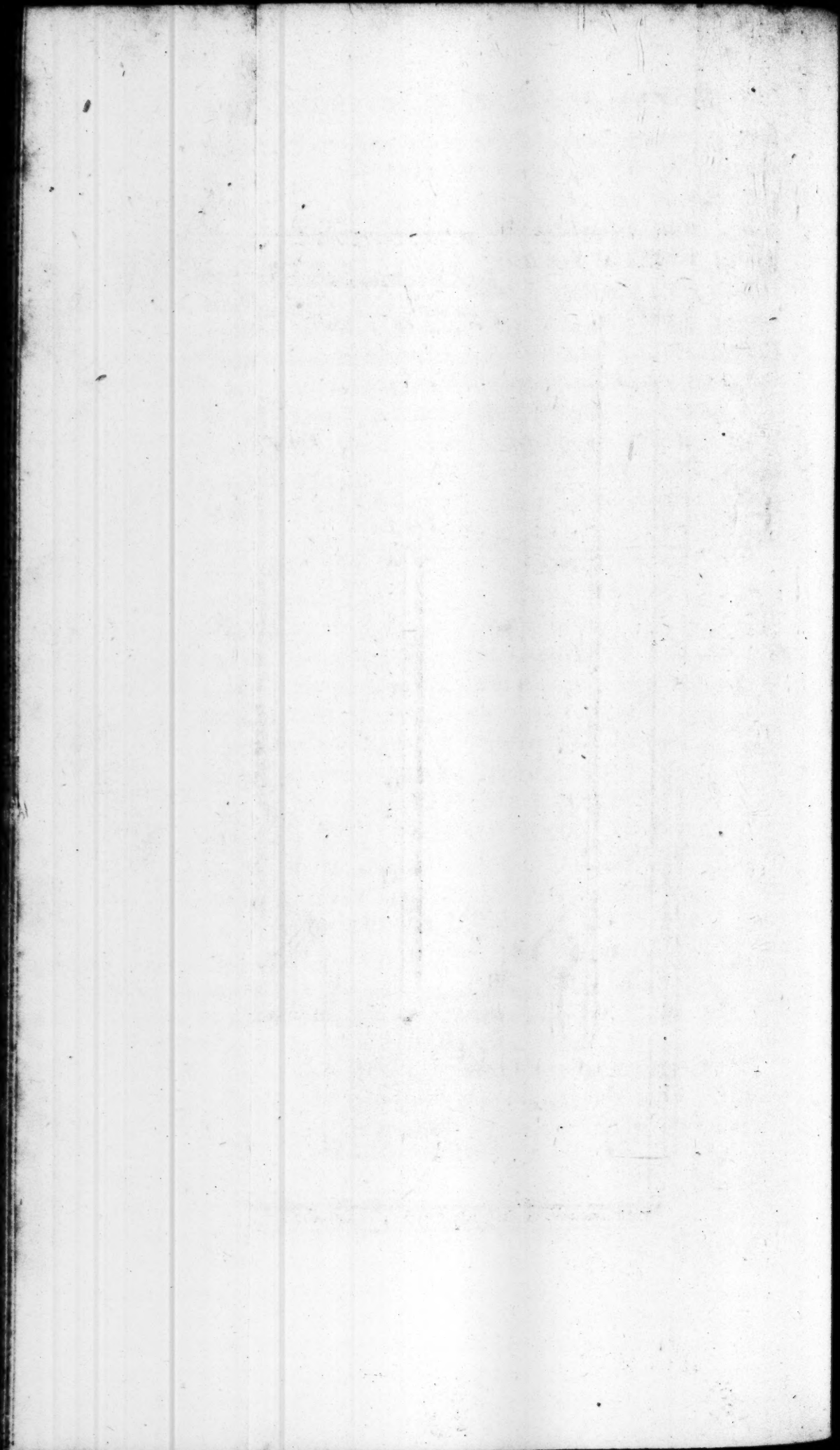
which I had observed only by my eye, and by putting my finger into it before it was foldered.

Now though these evacuations show, that this tube lets in the air; yet there are many things worth observation in this experiment. For first, the difference of the 4 inches, 4 lines, which was at first found between the heights of the quicksilver, contained at the same time in the iron and in the glass tube, cannot be imputed to the hole, through which the air must have time to insinuate itself into the tube; for, according to the observation of the continuance of these evacuations, it would have taken near two hours to let in all the air necessary to produce this difference, whereas it was instantly found.

Secondly, This experiment shews also, that the parts of quicksilver can by no means pass through such apertures, as the grosser parts of air pass through, when they are both equally loaded. Yet it is known, that quicksilver, when loaded, passes through very narrow passages, and the slowness with which the air penetrated into the iron tube, makes me conjecture, that the passage through which it passed, must have been a very little one. In the time of the evacuations, my thermometers were at 55 inches, 9 lines.

To conclude; it does not appear, that a reason can be easily given for this great difference in the heights of the quicksilver, otherwise than by supposing with me an inequality in the size of the parts of air which compose the atmosphere, and that the pores are greater in iron than in glass. Yet, as it is not known, whether the same thing would happen in other tubes of iron, I dare not conclude any thing thereupon, and look on this only as a preliminary experiment, which precedes those which are to confirm or explain it:





for, in short, perhaps the rust, which is considerable in the inside of this tube, retains many particles of air, which hinder the *vacuum* in this tube, from being made so perfect as in those of glass; which is yet what I can hardly believe, considering the care I took to get it out; and I cannot imagine, that a sufficient quantity should remain to produce such a considerable difference, independant of the pores of the metal.

I have said, in my last memoir, that spirit of wine perhaps only occasioned a less height in tubes which had been washed with it, because it made them cleaner, and hindered the dross of the quicksilver from fixing to them.

On this occasion, it would not be from the purpose for me to relate some experiments thereupon, which have shewn me, that the cleanest quicksilver, which has been long shaken in a very clean glass, sullies and obscures it very considerably. For having often carried in my pocket little bottles of quicksilver, some of which were even sealed hermetically, having carried them for a year or longer, I always found the bottle not only very dirty, but the quicksilver partly reduced to a black dust, like a pounded coal, as the company may have observed in that which I have long made use of, in form of one of those levels, called *ball levels*, in which the opposite sides are seldom parallel; though it is necessary to make the use of it certain, which is not so in this case.

But to return to our subject, it is therefore very possible, that the matter which passes through the pores of the glass, which has hitherto been thought to be only that of light, finds more or less hinderance to its passage, according as the entrance of these pores is more or less entangled with a foreign matter,

such as may be the dross and leaden part of the quicksilver, or of some other matter floating in the air, capable of producing such an effect; as it would happen to a fine sieve, exposed for some time to the smoak: for the soot which would cleave to it, might stop the holes; so that what before passed easily through it, could no longer pass at all, or with difficulty; and as by washing this sieve, it might be recovered to its first state. In like manner also, spirit of wine, or other liquors, may take off that kind of soot which hinders that passage to the little parts of air, which the greatness of the pores of the glass would perhaps otherwise allow them.

XX. *New reflections on the rules for the condensation of the air, by M. Cassini the son* †.

The perpendicular height of *Mount d'Or*, above the level of the sea, was measured to be 1040 toises, by the observations which we made on it, to determine the triangles of the meridian. The height of the quicksilver was there observed, by *F. Sebastien*, 22 inches, 2 lines, *June* 8. It was then at *Paris*, in the tower of the hall of the observatory, 22 inches, 9 lines $\frac{1}{2}$. There was a difference therefore of 5 inches, 7 lines $\frac{1}{2}$, to which, if we add 4 lines for the difference, which agrees with the height of the observatory above the level of the sea, we shall have for 1040 toises height of *Mount d'Or* above this level, 5 inches, 11 lines $\frac{1}{2}$ falling of the quicksilver, which is in the proportion of 14 toises, 3 feet, and some inches for each line, one with another. Accord-

† *Sepr.* 2, 1705.

ing

ng to the table *, which I have drawn up, according to M. *Mariotte's* rules, allowing with him for the first line of quicksilver, which answers to the level of the sea, 10 toises, 3 feet, we have for the line which answers to 6 inches diminution of quicksilver, which is nearly that which is found upon *Mount d'Or*, 13 toises, 2 feet, 2 inches, 2 lines less, than what is found for each line of quicksilver, even though we should suppose no augmentation caused by the dilatation of the air.

Continuing to compare the table, drawn by the rules with the experiments, we see that at 6 inches diminution of quicksilver, the height of the air above the surface of the sea, should be 852 toises instead of 1040, as has been found by observation, and that at the height of 1044 toises above the level of the sea, which is nearly that of *Mount d'Or*, there ought to have been found a diminution of 7 inches, 2 lines of quicksilver; that is, above 14 lines more than has been found by F. *Sebastien's* experiment, compared with that which was made at the same time at the observatory.

This difference is so considerable, that it cannot be ascribed to any error that could have been made in measuring the height of the mountains, nor to the different temperature of the air, which could have made a different variation in the height of the quicksilver at *Paris*; and at *Mount d'Or* we have found, that the variations in the height of the quicksilver usually happened at the same time; and if there were some differences, they never were near so considerable.

The observation, which F. *Sebastien* made at *Clermont*, gives us room to examine with more exactness the experiment, which M. *Perier* made

* See above, pag. 251.

on the *Puy de Domme*; and which M. *Mariotte* makes use of for the confirmation of his rules. *June* 10, 1705, F. *Sebastien* observed near the *Minims*, which is the very place where M. *Perier* made his experiments, the height of the quicksilver to be 26 inches, 6 lines. By the observations made at *Paris* before and after, it was 27 inches, 10 lines. The difference is 1 inch, 4 lines, which agrees with the height of *Clermont* above the observatory; to which if we add 4 lines, for the height of the observatory above the level of the sea, we have 1 inch, 8 lines, for the height of *Clermont* above the level of the sea. If we add to this difference 3 inches, 1 line $\frac{1}{2}$, which is that which M. *Perier* found between the *Minims* of *Clermont*, and the top of the *Puy de Domme*, we shall have for 812 toises, the perpendicular height of the *Puy de Domme*, above the level of the sea, determined by our observations, a diminution of quicksilver of 4 inches, 9 lines $\frac{1}{2}$. According to M. *Mariotte's* rules, the height of this mountain ought to be but 663 toises; and at the height of 812 toises, we ought to have found 5 inches, 9 lines diminution of quicksilver; that is, 11 lines $\frac{1}{2}$ more than we have found by the experiments. We shall find a still greater difference, if in the room of our observations, we use those which M. *de la Hire* has made at the observatory, which make the height of the quicksilver above a line lower than what we have observed. Here are several observations, made by various persons at different times, which all vary from the rules which M. *Mariotte* has established for the condensation of the air; we see also that his rules cannot answer exactly to experiments, whereas, according to the remarks which M. *Maraldi* read lately to the academy, there
is

is but one single observation, which varies about 4 lines from the rule, which he has laid down.

XXI. *An experiment on the quantity of heat reflected to us by the moon, by M. de la Hire jun. †; translated by Mr. Chambers.*

'Tis notorious that several persons attribute various qualities to the moon, without any reason for the same founded on experiments. I shall not here enter upon a detail of those qualities, as having observed, that most of those who give into the practice, differ in their sentiments, that which it should seem to have the most title to is heat, since its light is only that of the sun reflected, which every body will allow must occasion heat: however, as no experiments, that I know of, have been made, either to support or over-turn the reason upon which this quality is given it, I made the following one, with all the accuracy I was capable of.

In the month of *October*, 1705, the moon being in the meridian on the day of her opposition, and the sky very clear, I exposed the burning concave, of 35 inches diameter, kept in the observatory, to her body, and about the *focus* placed the ball of an air thermometer of M. *Amontons's*, which is the most accurate of any we have known. The whole being so disposed, as that the ball, which is 2 inches in diameter, received all the rays gathered into the *focus* upon its whole surface; hereupon examining the height of the mercury in the tube, after it had stood some time, I found it nothing different from what it was

* Nov. 28, 1705.

before

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before, notwithstanding that the rays were collected into a space 306 times less than they naturally possess, and consequently should have increased the apparent heat of the moon 306 times.

Now, if such an experiment as this, where the rays of the moon are not crowded into the 306th part of their usual room, but made to cross and intersect each other, which must considerably increase their effect, as is found by exposing the same concave to the sun, give us no signs of heat, we may conclude it incapable of making any sensible impression upon us.

A T A B L E

O F T H E

PAPERS contained in the ABRIDGMENT
of the HISTORY and MEMOIRS of the
ROYAL ACADEMY of SCIENCES at
PARIS, for the Year MDCCVI.

In the HISTORY.

- I. **O**N an irregularity of some barometers.
- II. **O**n the declination of the needle.
- III. *Why a glass vessel filled with water, or quick-silver, breaks before the fire in winter, but not if it is filled with spirit of wine.*
- IV. *Of some dogs almost starved to death.*
- V. *Of a man continuing fifteen days alive under the ground.*
- VI. *Of an extraordinary stone brought from Germany.*
- VII. *Of an accidental mixture of drugs, which produced a smell of musk.*
- VIII. *Of the formica-vulpes.*
- IX. *On the crystalline humour of a serpent's eye.*
- X. *Of a worm, which seems to have two heads.*
- XI. *Of an insect fish.*
- XII. *Of some extraneous fossils, found in Germany.*
- XIII. *Of an egg within an egg.*
- XIV. *Of the stomach of a dog found in the thorax.*
- XV. *Some inventions of M. Delesme.*

In the MEMOIRS.

- I. *Observations on the quantity of rain-water, which fell at the observatory, during the*
I year

- year 1705, and on the height of the thermometer and barometer.
- II. *Observations on the rain and wind, made in the year 1705, at the castle of Pontbriand, situated two leagues from St. Malo, in Bretagne.*
- III. *Other observations on the rain, which fell during the year 1705, at Lyons, communicated to M. Cassini by F. Fulchiron, a jesuit.*
- IV. *Observations on the barometer and thermometer, made at different places, during the year 1705, by M. Maraldi.*
- V. *Remarks on the bivalve shells, and first concerning muscles, by M. Poupart.*
- VI. *An extract of the observations made in December, 1705, by M. Bianchini, on some fires which are seen upon one of the Apennine mountains, by M. Cassini the son.*
- VII. *A dissertation on the barometers and thermometers, by M. de la Hire the son.*

A N
ABRIDGMENT
O F T H E

PHILOSOPHICAL DISCOVERIES and OBSERVATIONS in the HISTORY of the ROYAL ACADEMY of SCIENCES at *Paris*, for the Year 1706.

I. *On an irregularity of some barometers;*
translated by Mr. Chambers.

SO many several opinions being started in the Academy, touching the phænomenon of the chancellor's barometer, the conclusion was, that new experiments should be made. *M. Maraldi* accordingly had made several; and they all seemed to confirm the sentiment of *M. Homberg*, who apprehended that the barometer above-mentioned stood lower than the rest, by reason, after filling it with mercury, it had been washed in spirit of wine, some particles of this remaining in the pores of the glass, might, when the *vacuum* was made, become extremely rarefied, and thus depress the mercury, either of themselves, or by means of the air contained in them, and disengaged by their rarefaction.

The result of *M. Maraldi's* experiments is as follows.— After washing the inside of a tube with spirit of wine, and wiping it several times with different pieces of linnen, the mercury usually stands at a less height than before; and in different experiments, the difference of heights varies from $\frac{1}{2}$ an inch to an inch $\frac{1}{2}$.

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If the tube be filled immediately after washing it, the mercury will stand lower than if the tube had been filled some hours later.

If a tube have been washed with spirit of wine, the mercury will stand lower in it, than if the same tube had been washed with brandy; and if it have been washed in brandy, the mercury will stand lower, than if it had been washed in water.

If the tubes washed with these several liquors be afterwards well wiped, and dried, the mercury will stand at the same height as before they were washed.

A tube washed with spirit of wine, will be sufficiently dried by standing several days in the air, provided it be not moist.

Tho' the outside of a tube be washed, and rubbed ever so much with spirit of wine, the mercury will maintain its usual height.

In a barometer, which had two cracks at its upper end, the mercury did not sink for two months; that is, it only sunk as in other barometers.

Upon making barometers with several different tubes, none of which appeared moist, the mercury stood at different heights; the greatest difference being $\frac{1}{6}$ of an inch; but upon drying the tubes wherein it was lowest, it rose to the same height as in the rest.

From all this, it is easy to conclude what care and precaution is to be taken in making a good barometer.— As to the theory, all that can be alledged is, either that the little drops of the fluid, wherewith the inside of the tube had been moistened, became extremely rarefied in the *vacuum*, so as to depress the mercury, or that the air inclosed in such fluid, disengaging itself there-
from

from by this rarefaction, were the cause of depressing the same.

The first opinion is the least probable, by reason if the spirit of wine sunk the mercury by its own means, it would sink it less than brandy, as being lighter than brandy; and brandy, being lighter than water, would likewise sink it less than water, which is contrary to experience. It would follow therefore, that, agreeably to the second opinion, there is either more air inclosed in spirit of wine, than in brandy; or that it disengages itself more readily from the former than the latter; and the same will hold in the comparison of brandy with water.

But the great difficulty is started by M. *Amon-ton*, still remains; and till this be removed, the hypothesis hitherto explained, how plausible soever, will only be an hypothesis.

II. *On the declination of the needle; translated by Mr. Chambers.*

That noble system of Dr. *Halley* concerning the variation of the load-stone, which has been laid down in the history of 1701 *, and verified by the academy, in the history of 1705, is now further confirmed by M. *Delisle*, who, having an exact journal of a voyage to *Guinea* and *America*, made by M. *de Marchais* in the year 1704, 1705, and 1706, has been at the pains of comparing all the observations relating to the variation, with Dr. *Halley's* chart; that chart having been made for the year 1700, the declinations are not to be found the same in the succeeding years, as laid down by him, but different a little therefrom; and this more or less according

* Vol. I. Page 245 of this abridgment.

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to the length of time, which diversity, provided it agree with Dr. *Halley's* system, is a full confirmation thereof.

Now this is what M. *de Lisle* has found; the curve line of no variation, drawn by Dr. *Halley* round the globe of earth, only differs from that produced by M. *Marchais's* journal, by its being about half a degree more to the westward, as accordingly it had always been expected to find a motion in this line.

From this term, the declinations, observed by M. *Marchais* do all increase towards the east, and diminish towards the west, with regard to those in Dr. *Halley's* chart; and the greatest difference, which indeed only occurs once or twice, amounts but to about 2° —in 4 or 5 years. This is a new instance of what had been observed before, *viz.* That the declination does not vary equally and uniformly in all parts of the earth.

III. *Why a glass vessel filled with water, or quicksilver, breaks before the fire in winter, but not if it is filled with spirit of wine.*

M. *Hamburg* says, that a glass vessel put before the fire in winter breaks if filled with water, and more easily if filled with quicksilver; but not at all if filled with spirit of wine. The reason of which he imagines is, that the matter of light passing with difficulty through water or quicksilver, and consequently partly stopped by this obstacle, gathers into the pores of the glass in too great a quantity, where the fire continually drives it, and dilating these pores too much, forces the spring of the glass, and thereby breaks it; whereas had it met with spirit of wine in the same vessel, which is more homogeneous to it, and which
it

it easily penetrates, it would not have had occasion to exercise this violence. The experiment must be made in winter, because the vessels which then pass from a cold air to great heat, are more disposed to break ; but the fire must not be so great, or they so near it, as to break on this account alone. M. *Homborg*, much in the same manner explains, why an empty vessel, unstopped, being suddenly warmed before the fire, commonly breaks if it is thick ; but not if it is thin. The thickness makes the matter of light dilate the pores of the surface towards the fire, much more than the inward surface ; and from this inequality of dilatation, all the rest evidently follows.

IV. *Of some dogs almost starved to death.*

A *Danish* bitch, ready to whelp, having been forgot, and shut up in a chamber of a country-house at the family's returning to *Paris*, was, at the end of 41 days, found alive, lying on a bed, but could not stand, and without any signs of madness. No remains of its puppies, or excrements, were to be seen, she must have fed on those, and possibly on her milk also, and even on part of the dimity of a quilt, which she had torn, and of the wool in it, which she had tumbled about. Food was given her, and she began to recover from her extreme weakness, when M. *P'Abbé Galois* related this history.

On this occasion, M. *du Hamel* mentioned another bitch, which was 6 weeks without eating, except the straw of a chair, which was in the place where she was confined ; she had also drank some water, and lived very long after.

V. *Of a man continuing fifteen days alive under the ground.*

M. *Maraldi* also relates on this subject, that in an earthquake at *Naples*, a young man continued 15 whole days under some ruins, and was not starved to death.

VI. *Of an extraordinary stone brought from Germany.*

M. *Geoffroy* shewed a stone brought from *Germany*, he knows not from what part thereof. It is marbled, very soft to the touch, and in a manner fat and saponaceous. It is like soft marble, or petrified soap. It was thought to be a dry, hardened, slimy earth. M. *Homburg* says, its nature consists in having a finer grain than marble, tho' it weighs less, having greater pores. He adds, as a proof of the fineness of its grain, that being pounded and dissolved in water, it thickens it, which marble does not. Its effects are very near the same as soap. M. *de la Hire* says, that at *Montmartre* there is such a stone among the banks of sand.

VII. *Of an accidental mixture of drugs, which produced a smell of musk.*

M. *Lemery* having bought of a druggist half a pound of *galbanum*, as much *sagapenum*, as much *bitumen judaicum*, and 4 ounces of *opopanax*, put all these drugs in his pocket, each wrapped up separately in little bags, except the *sagapenum* and *opopanax*, which were together, he was

much surpris'd, when he returned home, that every body thought he smelt of musk; for every one of these drugs separate has a stinking and very penetrating smell, except the *bitumen judaicum*, which, however, has no smell like musk; and these very drugs are made use of in medicines against the vapours, a disease which may be occasioned by musk, and such like smells. He examined all the bags after one another; they were all new, none had been used to wrap up musk, or smelled of it, but only of the drug which had been put therein. He brought them all near one another again, and they produced a smell of musk. M. *Lemery's* cloaths were pretty strongly perfumed with it, which lasted till next day.

VIII. *Of the formica-vulpes.*

M. *Poupart* hath in the memoirs of 1704 *, given the history of the *formica-leo*, we presuppose this for the understanding of what follows. A friend of M. *Carré's*, seeking for these insects in the country, found many of those holes which they so cunningly make; but most of them without a *formica-leo*, which made him think, they had been a prey to some animal, who were greater *lions* than they. He was soon undeceived, observing at the bottom of these holes little worms, of about 6 lines long, and $\frac{1}{2}$ a line broad. He took some of them and put them in sand, where he saw them make their holes in the manner of the *formica-leo*. He gave them ants, which the *formica-leo* so much loves, and they greedily seized them, covering them with half their body, for the other part remains buried in the sand. As they have not so much strength as the *formica-leo*,

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their

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their prey often escapes from them, and to recover it, they make use of the same stratagem, they build their pit more sloping, which makes the animal fall back. The *formica-leo* likes them very well when they are given to him; but this is no wonder, for they like to eat one another. These worms change into an insect, very like a gnat, except that it is longer and thicker. The observer calls them, *formica-vulpes*, to distinguish them from the *formica-leo*, and describe their cunning.

IX. *On the crystalline humour of a serpent's eye.*

The same friend of M. Carré's examining the crystalline of a serpent's eye, which was a line in diameter, found it, even with the microscope, to be perfectly spherical. As it resembles a blown lens, he had a mind to view objects through it, and found that it magnified them exceedingly, full as much as such a lens of glass would have done, but it was not so transparent; probably by reason of the membrane which covers the crystalline. Hence it is certain, that these animals must see objects incomparably greater than we see them.

X. *Of a worm, which seems to have two heads.*

The same observer of nature met by chance a worm, 2 inches long, and 1 line broad, and $\frac{1}{4}$ thick, of a pretty deep yellow, like ear-wigs, and 80 legs on each side. The head and tail differ so little in their figure, that it cannot be conjectured, which of the two extremities is the head.

head. Nor can it be distinguished by the animal's walking; for if we stop it, it does not turn on one side like others, but returns short upon its steps, going backwards; so that the part, which in its first motion was behind, becomes foremost in the second, and these two motions are with equal facility. Perhaps, this insect hath two heads, and two brains, as others have many lungs. However, its extremities terminate in a point, with two little horns, like to its legs, and about 1 line long. It is sprightly and very nimble; and the order with which it successively moves its 160 legs is admirable.

The philosopher, who observed it, cut it in two equal parts, each of which consequently had 80 legs, they both walked as nimbly as the whole animal; they endeavoured to hide themselves in some hole, and the observer having put water in their way, each of them went a little into it, but soon got out again. He again cut each part in two, and still all four walked, but more slowly; it often made contorsions, like the tails of serpents when they are cut off. The separated parts did not endeavour to join; when they were put together, they stuck a little by means of a viscous juice which issued from their wounds, but they did not agree in their motion.

XI. *Of an insect fish.*

This philosopher has also found an insect fish, which transforms into an adder-bolt. When it is in the water, it is near 2 inches long, of which the tail is two thirds, and is 4 lines broad in the middle, terminating in a point. It is flat underneath, and round above. In the other third of this animal's length, is its head and six legs. The adder-bolt, which comes out of it, is

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one of those which fly about standing waters, where they lay their eggs. Here is an animal, which from a fish, becomes a bird, different in all likelihood from those mentioned by M. *Poupart* in the memoirs of 1704* ; perhaps, by much observation, it may be found, that this change of habitation and of element is pretty common.

XII. *Of some extraneous fossils found in Germany.*

What has been related in the history of 1703†, of stones taken out of the *Veronese*, which inclosed plants and dry fishes, was confirmed by M. *Leibnitz*. He says, that in the country of *Brunswick* near *Osteroda*, in the county of *Mansfield* near *Eislebe*, and in many other parts of *Germany*, veins of slate are found very near horizontal, whereon are very exact and compleat representations of many sorts of fishes or plants, which appear of their natural length and breadth, but without any thickness. These lines are often described on a mixture of copper, which also contains silver. Some of these plants are not now known in those countries ; but they may be found among the figures of *Indian* plants.

M. *Leibnitz* conceives, that a kind of earth has covered lakes and meadows, and buried therein fishes and plants ; or that some muddy water, filled with earth, has covered them, or carried them away. This earth has since hardened into slate, and the length of time, or some other cause, has destroyed the delicate substance of the fish or plant, near in the same manner as the bodies of flies, or ants, which are inclosed in yellow amber, have been wasted, whereof nothing

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† Page 13 of the same.

palpable remains, but simple delineations only. The matter of the fish or of the plant being consumed, has left its form impressed on the slate, by means of the cavity which remained therein; and this cavity was at last filled up with a metallic matter, whether a subterraneous fire baking the earth into a slate, has expelled the metal that was mixed with it, or that a metallic vapour penetrating the slate, has fixed itself in these cavities. M. *Leibnitz* adds, that this effect may be imitated by a pretty curious operation. Take a spider, or any other such-like animal, and wrap it up in clay, leaving a hole open from the outside into the cavity. Put the mass into the fire to harden it; the substance of the animal will be reduced to ashes, which may be washed out by some liquor. After which pour into the hole melted silver, which being cold, within the mass will be found the figure of the animal, well enough represented in silver.

Many authors have called these sorts of representations of fishes, or of plants in stones, *sports of nature*, but that is only a meer poetick idea, which does not suit with such a philosopher as M. *Leibnitz*. If nature sported, it would be with more freedom; she would not confine herself to express so exactly the smallest features of the original; and what is more remarkable, preserve such true dimensions. When this exactness is not found, it may be sport; that is, dispositions which are in some sort accidental. It is true, that a representation of an *Indian* plant in a *German* stone, seems at first contrary to the system of M. *Leibnitz*. But that the plant represented should be found in the *Indies*, favours much the opinion, that there is no wantonness therein. It is easy to imagine many accidents, whereby a plant may

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have been brought from the *Indies* into *Germany*,
even in those times when there was no com-
merce between these countries by navigation; and
it appears by several marks, that there has been
great changes in nature on the surface of the
earth. M. *Leibnitz* thinks the sea once almost
covered the whole, and that afterwards a great
part of its waters made a passage into those hol-
low abysses which are within our globe. From
thence proceeds the shells of the mountains. But
the whole of this subject deserves a more ample
discussion.

XIII. *Of an egg within an egg.*

M. *Mery* shewed a hen's egg boiled, in the
white of which was inclosed another little egg,
covered with its shell and inward membrane, and
filled with a white matter, without yolk. As
this little egg had been given when boiled to M.
Mery, he could not observe whether it had a
sperm.

XIV. *Of the stomach of a dog found in the thorax.*

M. *Littre* dissecting a dog, was much sur-
prized to find his stomach in the thorax, and
placed above the diaphragm. Instead of the hole
through which the *æsofagus* crosses the dia-
phragm to go into the stomach, there was a
great fissure, the lips of which were cicatrized,
and seemed to have been a long time so, and in-
stead of the *æsofagus*, the gut duodenum went
through this hole. As it is always fastened to the
inferior orifice of the stomach, it followed it into
the cavity of the *thorax*, which it could not do,
without

without being stretched and flatted. *M. Littre* would see whether the stomach could pass thro' the fissure in the diaphragm, but it was too little; and after he had made an incision therein, the stomach fell into its natural place, and the *æso-phagus* was long enough not to hinder it, which shews that the stomach had at first been in its right situation, but some violent accident had removed it by a rent or slit in the diaphragm, which afterwards shrunk when it cicatrized.

But what could this accident be? *M. Littre* guesses two, either an extraordinary convulsion of the *æso-phagus*, which by contracting had drawn the stomach towards it; or an extreme contraction of the diaphragm, and at the same time of the muscles of the belly; for these forces being opposite, the stomach, thrust downward by the diaphragm, and upward by the muscles of the belly, must go upward, even tearing the diaphragm, whose resistance is naturally less than the force of the muscles, which act against it. It may also be supposed, that this stomach was then full of solid food, which must have added strength to its action against the diaphragm.

However, this stomach being now not placed between the diaphragm and the muscles of the belly, these muscles could no longer, as usual, concur with it by their contraction in pounding the food, and driving it into the guts. They must therefore remain too long in the stomach, and thereby contract some vice. Besides the *duodenum* being stretched and flatted, did not give a passage free enough to the digested food. In short, the heart and lungs were much disordered in their motion by this stomach, which took up its lodging in the *thorax*. From all which it may be easily concluded, was it no otherwise known;

known ; that the animal breathed with difficulty, and palpitation of heart ; that the more he eat, the greater must be his uneasiness ; that he must frequently have ineffectual inclinations to vomit, and be always very lean.

XV. *Some inventions of M. Dalefme.*

We have barely mentioned in the history of 1705, that M. *Dalefme* had given a very plain way to facilitate and augment the action of those who tow great boats. It is to hang along a river's side a rope, which the men may take in their hands as they tow the boat : he has found experimentally that with this assistance, they have almost half as little trouble ; but the trial was made only in a little way, it is likely, that in a longer, they would be more tired with using the rope, than in towing after the usual manner, their arms being in action as well as their legs and feet. The remedy for which would be to make use of the rope only in difficult places, and then let their arms rest.

Here are two other thoughts also proposed by M. *Dalefme*.

1. To melt leaden pipes for the conveyance of water without folder, and at one attempt, and then to pass them through a drawing-iron with a mandrel in the pipe. There are two sorts of common pipes ; the one made of sheet-lead rolled into a tube, and afterwards foldered ; this foldering is often faulty and leaks : the other sort are melted and run, but at several attempts, the molds being not long enough to make them at once, therefore they are foldered together by melting ; but this cannot be done so perfectly, but that many little holes remain, and therefore the
metal

metal is beat with a hammer. But pipes, which are cast in M. *Delafme's* manner, will not have the inconvenience of the first, which require to be soldered their whole length; and if they happen to have holes, or pores, they will close better than in the second sort; for the drawing-iron forcing and squeezing the metal, if I may use that expression, will bind it much tighter, and more equally than the hammer. Milled metals are more strong, and less porous than those which are only cast, and drawing them through the iron, is better than milling them.

2. To fasten to great ships with a sort of pitch or cement, which serves to careen them, sheets of lead, at first cast thick, and afterwards milled thin, which is consequently very solid, and has but few pores. Ships would hereby be secured against worms, which in hot climates eat and destroy them. The lead would be 4 or 5 times less expence than the double planks with which they are usually sheathed; nor will it loosen from the vessel, till the cement comes off.

A N ABRIDGMENT

OF THE

PHILOSOPHICAL MEMOIRS of the ROYAL
ACADEMY of SCIENCES at *Paris*, for
the Year 1706.

I. *Observations on the quantity of rain-water, which fell at the observatory during the year 1705, and on the height of the thermometer and barometer, by M. de la Hire*.*

I Made observations on the quantity of rain-water which fell at the observatory throughout the year 1705, in the same manner as on the preceeding years. I found the height of rain-water in the month of

	<i>Lines.</i>		<i>Lines</i>
Jan.	5 $\frac{3}{4}$	July	2 $\frac{3}{4}$
Feb.	8	Aug.	19
Mar.	7 $\frac{1}{8}$	Sept.	16 $\frac{1}{8}$
Apr.	23 $\frac{3}{8}$	Octob.	27 $\frac{7}{8}$
May	4 $\frac{1}{2}$	Nov.	13 $\frac{1}{4}$
June	15 $\frac{1}{2}$	Dec.	23 $\frac{1}{2}$

The quantity of water in height has therefore been this year 166 lines $\frac{1}{4}$, or 13 inches, 10 lines $\frac{3}{4}$, which is not much more than two thirds of what commonly falls; which, by the comparison for many years, I have esteemed to be 19 inches. I have not met with so great a drought as this, during the many years I have made these obser-

* June 9, 1706.

vations;

vations: yet has the harvest been plentiful enough, which may be attributed to the great rains in the month of *April*, which sufficiently supplied the earth with moisture against the following drought.

The three summer months, which commonly furnish as much water as all the rest of the year, by reason of storms and pouring rains, gave but 37 lines $\frac{1}{4}$, and *July* furnished but 3 lines. Nor was it the great rains of this season, which contributed to the fruitfulness of the earth; for they rise again in vapours almost as soon as they fall, and part of them run off without penetrating deep.

The winds were in *January* very changeable. All the beginning of *February* they were easterly, sometimes inclining to the N. and sometimes to the S. and at the end towards the W. In *March*, the wind was almost always at the E. sometimes passing to the N. and sometimes to the S. In *April*, the governing wind was about S. W. In *May*, the wind reigned in the N. sometimes varying towards the W. In the first half of *June*, the wind was as in *May*, and without rain; but the 14th, it began to rain till the 17th with a N. wind afterwards N. W. and S. W. and there fell 11 lines of water, and the 22d, 4 lines, with a little storm: the rest of the month, the wind was N. and N. E. In *July*, the governing wind was N. and in this month there was very little rain. In *August*, the wind was almost always W. and S. W. In *September*, the governing wind was W. varying a little to the S. and N. In *October*, the wind was often N. inclining sometimes to the E. and to the W. In the first half of *November*, the wind was N. and N. E. and in the other half S. W. and N. W. In *December*, the governing wind was very high at S. and S. W.

with very great violence, and a sort of hurricanes rising two or three times. The 3d of this month in the evening, the wind was very high in the S. with thunder; which, in this country, seldom happens in the winter.

There has been no snow this year.

The thermometer with spirit of wine sealed hermetically, which I make use of to measure the cold and heat, indicated, that the cold in the beginning of the year was not considerable, for this thermometer fell only to the 25th degree *February* 2, and to 30 degrees *November* 13, when it froze pretty hard, and immediately it rose again to 40 degrees. It always begins to freeze in the country, when it is fallen to 32. Its mean state, as it is in the bottom of the caves of the observatory, is 48 degrees. These caves are 14 toises under ground, and very near the level of the river, when it is at a mean height. In the morning, at the sun-rising, the greatest height of the thermometer was at 65 degrees $\frac{1}{2}$, *Aug.* 18; but near 3 in the afternoon, when the air of the day was hottest, the thermometer rose at the end of *July*, and the beginning of *August*, to 75 degrees; and *August* 6, it was at 80 degrees, tho' it is shaded and exposed to the air in the open tower of the observatory, which shewed an excessive heat; and I question, whether it has ever been greater in this country. And most of the thermometers with spirit of wine broke, the liquor contained therein having not had room enough to rise in the top of the tube, which did not happen to mine, having made it very long, that I might expose it to the sun.

It ought to be observed, that the greatest heat of the afternoon does not always answer to that in the morning, through several particular reasons.

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By these observations, it is also seen, that the heat has this year been in a greater proportion than the cold ; for the thermometer exceeded its mean state in the heat 32 degrees, and in the cold it fell below it only 23 degrees.

These are the observations on the weight of the air, which the barometer has shewn us. In that which I commonly make use of, which is one of those called simple, and is always placed at the height of the great hall of the observatory, the quicksilver rose highest to 28 inches, 3 lines $\frac{1}{3}$ Feb. 28, with a small N. N. E. wind, and it fell at lowest to 26 inches, 7 lines $\frac{1}{6}$ Dec. 20, with a W. S. W. wind ; thus the difference of a height between the lowest and highest was 1 inch, 7 lines $\frac{1}{2}$, much as usual.

The great elevation of the quicksilver in the tube of the barometer seems commonly to be only when the wind is northward ; and on the contrary, it almost always falls lowest when the wind is southerly, and when it is violent and tempestuous ; yet there are particular causes which may make the air heavier or lighter, tho' the wind is neither northward or southward. Therefore the observations of the barometer are not to be too much depended upon concerning what the weather will be.

I shall here observe by the way, that there are some barometers in which the quicksilver rises much higher than in others, at the same time and at the same place, though they are made with great care. In that which I commonly make use of, the observations which I have now related, the quicksilver is always 3 lines $\frac{1}{2}$ lower than in another of mine, which is that wherein the light was first observed in the vacuum at the top of the tube, by agitating the quicksilver,

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though in the other there appears to be some also. But if in the thermometer, wherein the quicksilver does not rise so high, the gross air has not been so exactly evacuated as in the other, by reason perhaps of the great length of its tube, it will follow, that it is not necessary that the quicksilver should be exactly cleansed from air to make the light appear in the *vacuum*. In short, after all the experiments we have made on barometers with different quicksilver in the same tube, and different tubes with the same quicksilver, it seems necessary to believe, that the different heights of the quicksilver in the tubes of barometers, proceed only from the nature of the glass, whose pores are not equally close, and that the air is not only composed of two different matters, one of which is altogether gross, and the other subtile; but that the particles of the gross matter differing in their size, even till they become subtile, the pores of some glasses suffers this less gross matter to pass, which by its weight makes the quicksilver in the tube fall a little. This hypothesis seems confirmed by one of the last experiments, made by M. *Amontons* at the academy, with the barrel of a gun well foldered at one end, instead of a glass tube, wherein it was observed, that the quicksilver stood much lower than in a common glass, perhaps because the pores of iron are much greater than those of glass.

I observed the declination of the needle $9^{\circ} 35'$ towards the W. the last day of *Dec.* 1705, with the same needle and in the same place, where I have observed it many years, and with the same circumstances, as I have related in other memoirs.

II. *Observations on the rain and wind, made in the year 1705, at the castle of Pontbriand, situated two leagues from St. Malo in Bretagne **.

The observations on the rain and wind, made at the castle of *Pontbriand*, two leagues from *S. Malo* in *Bretagne*, may be seen in the memoirs of the year 1704.

This is the continuation of these observations, made by *M. Pontbriand* with great exactness in the same place during the year 1705, sent by him to *M. du Torar* of the royal academy of sciences, in order to be communicated to the academy, and compared with the observations made at *Paris*, by *M. de la Hire*.

JANUARY.

Days.	Rain-water.	Winds.
	Lines.	
3	2	North N.E.
10	2	S.S.E.
11	1½	S.S.W.
12	1½	W.N.W.
13	3½	N.N.W.
14	1½	W.N.W.
17	1½	E.N.E.
19	0½	E.N.E.
Total 14 lines.		

* Jan. 9, 1706.

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FEBRUARY.

<i>Days.</i>	<i>Rain-water.</i>	<i>Winds.</i>
	<i>Lines.</i>	
5	2	East S.E.
10	1	N.N.E.
13	$0\frac{1}{4}$	N.W.
14	2	N.N.W.
16	$2\frac{1}{2}$	S.S.W.
17	$0\frac{1}{2}$	S.S.W.
18	$2\frac{1}{2}$	E.S.E.
25	$1\frac{1}{2}$	E.N.E.
28	$0\frac{1}{4}$	E.N.E.

Total 13 lines.

MARCH.

<i>Days.</i>	<i>Rain-water.</i>	<i>Winds.</i>
	<i>Lines.</i>	
4	$3\frac{1}{2}$	North-East.
8	$0\frac{3}{4}$	S.S.W.
10	$0\frac{1}{2}$	S.S.W.
13	$1\frac{1}{2}$	S.S.W.
14	$0\frac{3}{4}$	S.S.E.
24	$1\frac{1}{2}$	S.E.
25	$1\frac{1}{2}$	S.E.
26	$3\frac{3}{4}$	S.S.W.
27	5	S.S.W.
28	$0\frac{1}{2}$	S.S.W.
29	$3\frac{1}{2}$	S.E.
30	$0\frac{1}{2}$	N.E.
31	$0\frac{1}{2}$	N.E.

Total $23\frac{2}{4}$ lines.

APRIL

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A P R I L.

<i>Days.</i>	<i>Rain-water.</i>	<i>Winds.</i>
	Lines.	
1	0 $\frac{1}{2}$	West.
2	1 $\frac{1}{2}$	N.W.
3	0 $\frac{1}{4}$	N.W.
4	2	N.W.
5	2 $\frac{3}{4}$	W.N.W.
7	3 $\frac{3}{4}$	W.S.W.
8	2 $\frac{3}{4}$	W.S.W.
9	1 $\frac{1}{2}$	S.S.W.
10	0 $\frac{3}{4}$	S.S.W.
11	1	N.W.
12	1	N.W.
15	1	N.W.
16	0 $\frac{1}{4}$	S.S.W.
19	1 $\frac{3}{4}$	N.E.
20	0 $\frac{1}{4}$	S.S.E.
24	4 $\frac{1}{2}$	N.E.
25	0 $\frac{1}{2}$	N.W.

Total 26 lines.

M A Y.

<i>Days.</i>	<i>Rain-water.</i>	<i>Winds.</i>
	Lines.	
4	3	West.
5	0 $\frac{3}{4}$	N.W.
18	4	N.W.
26	0 $\frac{3}{4}$	S.E.
28	0 $\frac{1}{4}$	N.E.

Total 8 $\frac{1}{4}$ lines.

J U N E.

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J U N E.

<i>Days.</i>	<i>Rain-water.</i> Lines.	<i>Winds.</i>
13	$4\frac{3}{4}$	South-S.W.
16	$0\frac{1}{4}$	W.S.W.
17	2	W.S.W.
Total $7\frac{1}{2}$ lines.		

J U L Y.

<i>Days.</i>	<i>Rain-water.</i> Lines.	<i>Winds.</i>
2	$0\frac{1}{4}$	North.
3	1	N.
6	$2\frac{1}{2}$	N.W.
10	$0\frac{1}{2}$	N.W.
Total $4\frac{3}{4}$ lines.		

A U G U S T.

<i>Days.</i>	<i>Rain-water.</i> Lines.	<i>Winds.</i>
7	$2\frac{3}{4}$	South-West.
21	3	W.S.W.
23	$1\frac{1}{2}$	S.W.
24	1	N.W.
27	$2\frac{1}{2}$	E.S.E.
31	1	W.S.W.
Total $11\frac{3}{4}$ lines.		

SEP-

SEPTEMBER.

Days. Rain-water. Winds.

	Lines.	
3	$2\frac{1}{4}$	North-West.
4	$2\frac{1}{2}$	N.N.W.
5	2	W.S.W.
6	$3\frac{1}{2}$	W.S.W.
8	3	N.W.
10	$1\frac{1}{4}$	S.W.
15	$0\frac{1}{2}$	N.W.
23	3	N.N.W.

Total 18 lines.

OCTOBER.

Days. Rain-water. Winds.

	Lines.	
6	$5\frac{1}{2}$	North-West.
12	1	N.W.
13	1	N.W.
14	$0\frac{1}{2}$	N.W.
15	2	S.S.W.
16	$0\frac{1}{2}$	S.S.W.
17	$3\frac{1}{2}$	S.S.W.
18	$6\frac{1}{4}$	S.S.W.
19	3	S.S.W.
29	2	S.S.W.
30	3	N.W.
31	3	N.W.

Total $31\frac{1}{4}$ lines.

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NOVEMBER.

Days.	Rain-water.	Winds.
	Lines.	
2	3	East.
3	$2\frac{1}{2}$	N.E.
4	1	N.E.
9	$0\frac{1}{2}$	N.E.
15	$1\frac{1}{2}$	E.S.E.
18	$1\frac{1}{2}$	N.E.
20	1	N.W.
21	$0\frac{3}{4}$	S.W.
22	$0\frac{1}{2}$	N.W.
25	$3\frac{1}{2}$	N.W.
26	$0\frac{3}{4}$	N.W.
28	$0\frac{1}{2}$	N.N.W.
29	5	S.W.
30	$4\frac{1}{2}$	N.W.

Total 26 lines $\frac{1}{2}$.

DECEMBER.

Days.	Rain-water.	Winds.
	Lines.	
1	1	North-West.
3	5	N.W.
4	$5\frac{1}{4}$	W.S.W.
5	$2\frac{1}{2}$	N.W.
6	$1\frac{1}{2}$	S.W.
10	$3\frac{1}{2}$	S.W.
13	$2\frac{1}{2}$	S.S.W.
14	1	S.S.W.
15	$8\frac{1}{2}$	S.S.W.
16	3	N.W.
17	$2\frac{1}{2}$	N.W.

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Days.	Rain-water, Lines.	Winds.
18	1 $\frac{1}{4}$	North-West.
19	7	W.S.W.
20	3 $\frac{1}{2}$	N.W.
21	2 $\frac{1}{2}$	N.W.
22	5 $\frac{1}{2}$	S.W.
24	3	W.S.W.
25	3	W.S.W.
26	4	W.S.W.
29	1	S.S.W.
* 30	5	S.S.W.
31	5	S.S.W.

Total 75 lines.

Total quantity of the rain-water, which fell
at Pontbriand, Lines.

During the year 1705. 260

And in the year 1704. 284

Difference 16

* On the 30th, there was a violent tempest, which made
great confusion in *Bretagne*.

III. *Other observations on the rain, which fell during the year 1705, at Lyons, communicated to M. Caffini by F. Fulchiron, a jesuit.**

	Lines.		Lines.
Jan.	7	July	12 $\frac{3}{4}$
Feb.	27	Aug.	15
March	18 $\frac{1}{2}$	Sept.	6 $\frac{1}{4}$
April	10 $\frac{1}{2}$	Octob.	47 $\frac{3}{4}$
May	23 $\frac{1}{2}$	Nov.	14 $\frac{3}{4}$
June	26 $\frac{1}{2}$	Dec.	63 $\frac{1}{2}$

	Lin.	In. L.
Sum total	272 $\frac{1}{2}$,	or 22 8 $\frac{1}{2}$
In the year 1704 it rained		15 4 $\frac{1}{2}$
Difference		7 4

IV. *Observations of the barometer, and thermometer, made at different places during the year 1705, by M. Maraldi †; translated by Mr. Chambers.*

The barometer in the western tower of the observatory was at its greatest height on the three last days of *February*, when we found it at 28 inches, 3 lines $\frac{1}{2}$, the wind blowing at N. and N.E. its least height was 26 inches, 8 lines $\frac{1}{2}$, at which we found it on the 19th of *December*, the wind blowing violently at south and south-east, with rain, the difference therefore or play of the barometer this year was 1 inch, 7 lines.

The thermometer made by M. *Amontons*, situate in the western tower of the observatory, was found on the 2d and 3d of *Feb.* at 51°, 11 lines,

* Jan. 16, 1706.

† The same date.

which

which was its least height.— By the observations made at *Genoa*, by the marquis *Salvaga* with a thermometer, like ours, the same 3d of *Feb.* was also the time when it was lowest at that city, being there at $52^{\circ}, 10$ lin. which is almost a degree higher than at *Paris*.—— By the observations made at *Lyons*, the thermometer was also found at its lowest pitch for this year on the same 3d of *February*.

At *Paris*, the thermometer was found at its highest pitch for this year on the 6th of *Aug.* when it rose to $57^{\circ}, 3$ lines, the wind blowing at south-east. On this day a thermometer of M. *Cassini*, which had stood 35 years, burst, by reason the liquor rose all into the tube, where wanting room for other rarefaction, the glass was forced to give way.—— At *Genoa*, a thermometer of M. *Amontons* rose on the 2d and 3d of *August* to its highest pitch for this year, which was $56^{\circ}, 8$ lines, the wind blowing at north; so that the thermometer did not rise so high at *Genoa* this year as at *Paris*, by $\frac{1}{2}$ a degree.— At *Lyons*, the thermometer rose to its state on the 8th of *August*, two days later than at *Paris*.— At *Montpellier*, M. *Bon* found one of M. *Amontons* thermometers rise to its highest state, which was $58^{\circ}, 2$ lines, on the 30th of *July*; so that at *Montpellier*, it was almost a degree higher than at *Paris*, the vehement heat of this day burnt most of the vines; and it was observed, that upon exposing the thermometer $28'$ — to the naked sun, it rose to its highest pitch possible; that is, to 75 inches, which is the degree laid down by M. *Amontons* for the heat of boiling water.

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V. *Remarks on the bivalve shells, and first concerning muscles, by M. Poupert *; translated by Mr. Chambers.*

Muscles are a kind of little fishes, lodged between two shells, which are usually concave and convex.

There are both sea and river muscles; the latter are divided into several species; some whereof we shall take occasion to consider hereafter.

They all open, shut, walk, and some of them float on the water. They all likewise creep half out of their shells, and back again. Add that all of them shed their milt, and breathe, or rather draw in the water with their gills, and hide themselves in the sand or mud alike.

How they open their shells.

Shell-fishes, in all probability, were the first fishes that mankind knew, or bethought themselves of eating; for it was a good while ere the line, hook, net, and other utensils of fishing were invented. But for the shell-kind, the sea spontaneously casts them ashore; so that nothing was required of the first people, but to stoop and take them up. The manner, however, of their opening has not been yet discovered; tho' a very able *Dutch* anatomist has sought it with great pains, as appears in his treatise upon the anatomy of the muscle. This shews, that the simplest and least intricate things are sometimes the most difficult to discover.

All the kinds of muscles, and even all shell-fishes with a double shell, have a sort of leather ligament, which binds the two shells together at

* Feb. 20, 1706.

the posterior and thicker part, called the *heel* thereof. And 'tis by the spring of this ligament, that the shells are opened. This ligament is the more extraordinary, as it has two seemingly opposite effects, both to keep the shells close together, and to open them. The manner hereof is as follows.

When the muscle, or other fish, closes its shell, by contracting the muscles thereof, the ligament, which is between the edges of what we call the heel, becomes compressed of course; and thus remains so long as the muscles remain in a state of contraction. But this ligament, tho' very hard, is yet somewhat spongy; so that coming to swell, upon a relaxation of the muscles, it thrusts the two shells asunder, and makes them open a little.

Different kinds of fishes have different ligaments, that of the river muscle is a kind of hinge fastened externally on the edge of the two shells, and reaching outwards. It would not be well inclosed between the edges of the shells, tho' since in such as have a ginglymus to be mentioned hereafter, it would cover and render it useless, and in such as have none, the edges are too thin to contain the whole ligament.

The ligament of the sea-muscle differs from the river-kind, in that it is not fastened behind the shells, but partly between the edges, so as none of it comes without, but a little of it hangs over within the shell, by reason the edges are not thick enough to hold it all. To supply in some measure this defect, it is surrounded with two strings, strongly fastened on the inner edges of the shell, whose thickness is increased hereby. These strings are hard and porous, and seem super-added to the shell, and of a different substance,

'tis

'tis probable, that the tracks graven in them are not altogether useless, tho' I have not yet discovered what purpose they may serve. The office of the strings themselves, is to give thickness to the edges of the shell, and hereby enable them better to compress the ligament between them, which the bare edges of the shell could not do, as being too thin, and consequently their compression too weak to occasion a spring sufficient to open the muscle. — The ligament that opens the oyster's shell is very different from that of the muscle-kind; it does not enter at all within the cavity of the shell, as that of the sea-muscle does; nor does it reach without it, like that of the river-muscle, but is inclosed in the heel between the shells, where there is room enough to hold it.

It is shaped like a crescent, whose back or thickest part is turned towards the cavity of the shell, the horns or slenderest parts looking outwards, and the middle space filled with a fungous matter. Hence the shells finding most resistance in pressing upon the thickest part, its spring or elasticity must be greatest on that side the shells are to open on. It must be added, that this ligament does not reach to the tip of the heel, but leaves a little vacancy there, that the shells may have a liberty to open.

The matter of this ligament in oysters is not altogether the same with that of muscles, but more coriaceous and dry. The ligament of the sea and river-muscle is very stiff and brittle; so that if left a little out of water, it breaks upon the least opening or shutting of the muscle. There is indeed a necessity for its being dry; for being always in water, it would otherwise be so softened, as to loose all its spring, whereas now it only softens like a strong piece of leather, and they

easily bends or straitens again, without breaking when the contraction or relaxation of the muscle requires it, even the whole muscle may be opened without breaking the ligament.

It would be curious enough to examine the several ligaments which open the various sorts of shell-fishes, no doubt many things would be found therein very singular and extraordinary. This I note in favour of such as take delight in unfolding the smallest mysteries of nature, whose curiosity should always have something left it to be employed upon.

The manner how muscles shut.

All muscles close their shells by the contraction of two large fibrous muscles fastened within side each end of the shells, which muscles are too well known to need further description, I shall only add, that the shells shut so close, that water can hardly pass them: the manner of their shutting is as follows.

The shells of all muscles are bordered round with a kind of membrane, which may be called their *epidermis*, as being a continuation of the exterior *stratum* of such shells. These membranes fit so close to each other, that when moistened, the smallest drop of water cannot escape from the muscle, without an expedient of this kind, it had been difficult so to form the edges of hard thin brittle shells, as to render them impervious to water.

Beside this membrane round the inner edge of each shell runs a ligament, which bearing one against another, when the shells are shut, proves likewise an obstacle to the water's escaping, and even hinders the edges of the shells from breaking by the violent contraction of the muscles.

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Some muscle-shells are not only bound together by the contraction of their muscles, and by the springy ligament above-mentioned, but likewise by long grooves or furrows, which receive a sort of tongues, or slips in their whole length. At the end of these grooves, immediately under the heel, is a dented pin, which enters a dented cavity of the other shell; on the edges of which cavity are two little dented eminences, which are likewise received in two little dented cavities of the other shell, the dentile of the epiphysis and cavities thus mutually receiving each other like those of the cranium.

But this ginglymus is not found in all the sorts of muscles; the sea-muscle, that larger kind found in ponds, which sometimes grows a foot long, and that other which I denominate the crested kind, from an eminence in form of a cock's comb near the heel, have no such articulation.

Of the progressive motion of muscles.

From the structure of muscles, one would imagine they should have no other motion but what they receive from that of the water; and yet they all walk, and some of them float on the surface of the water; their manner of walking is thus:

Being laid on the flat side of their shells, they issue partly out of the same, in figure of a tongue, and make little vibrations therewith, to dig the sand or mud of the river. In thus digging, they dip gradually on one side, till at length they are found on the edge of the shell, with their back or heels upwards. In this state they gradually advance their head; that is, the tip of the tongue above-said, for a minute or two, and then bear or rest upon it, in order to draw the shell towards the same, much as water-snails are sometimes seen

seen to do. They repeat this motion so long as they are disposed to walk; and thus form a sort of irregular tracts, which are sometimes 3 or 4 yards long, in which they lay half hid.

In the summer time, we find store of these tracks in rivers where muscles abound, and never miss of a muscle at the end of each tract. 'Tis in this manner these little fishes seek their living, walking this way and that, with the heel foremost, and still plowing the ground with the edge of their shells.

These hollow tracks serve as rests to sustain the muscles on the edge of their shells, and may likewise probably serve to catch and detain the spawn of some fishes, or other kinds of food proper for them.

One would think it had been better, if the point of the shell had gone before the heel, since being thinner and sharper, it would have been fitter to dig the ground, like the share of a plow, which always goes with the point foremost.

I do not find any muscular structure, whereby the muscle should be pulled out of its shell, which leads me to imagine, that this is only done by glutting and dilating themselves with water. This they will swallow so plentifully, that I have drawn half a glass full from the pond muscle.

One of the most considerable effects of their walking is, that by means thereof, they are able to meet and rub or spawn together.

I have found no eggs in any muscles, yet in the summer-time, there is both milt and store of mucus in the same muscle, which makes me conjecture, that they may be hermaphrodites.

The large gland of the crest of the muscle is always full of a very white milt in the month of September, in which milt there is this remarkable, that

it coagulates as soon as thrown in water. This coagulation makes me imagine, that the muscle does not cast its spawn in the water, since it would hereby be rendered useless for generation. I rather suppose therefore, that the muscle insinuates its milt into another muscle, at the time of propagation. There is even reason to suppose, that the same happens in other fishes, and that they do not cast their milt in the water, as is vulgarly imagined.

To see this milt, the large gland of the crested muscle, which makes the firmer and better part of the fish, must be cut in two, upon which a flux thereof will be seen, as if the whole had been melted down, gathering this milt with a knife-blade, and presently casting it into the water, it instantly coagulates into little grumes or clots.

Of the fluttering of a kind of muscle.

Aristotle tells us, that he had been informed of a large sort of muscle, which flutters in the water; and we have already observed, that the philosopher was not deceived. For I have had the good fortune to see the pond-muscle above-mentioned fluttering after this manner. — The reason I take to be this.

This muscle has a very thin light shell, yet so large that it may beat the surface of the water, as birds do the air with their wings. On the back of the shell is a large springy ligament, and within are two strong muscles to shut it. This is enough to make it flutter; all required to that end, being that these springs act readily one after the other, and strike the air with sufficient velocity and strength. What favours this motion still more is, that the ginglymus found in other shell-

shell-fishes which do not flutter, is not seen in this, which would prove an impediment.

The manner how muscles bury themselves in the sand.

When the muscles find it cold they bury themselves in the sand, and to that end they detach a part out of their shells in figure of a tongue, which they drag gently this way and that to move the sand; till in half an hour's time they become wholly covered therewith.

How the muscles re-enter their shells.

The muscles are enabled to return in their shells by means of a muscular membrane, where-with the large gland, already observed to issue from the shell in figure of a tongue, is wholly invested. When this membrane contracts the gland, which is naturally soft and flexible, becomes hard and wrinkled, after handling it a little, as we find in snails on the like occasion.

Of the ejaculation of its milt.

There is a deal of probability, that it is the contraction of the muscular membrane above-mentioned, that occasions the milt to issue from the large gland through little pores, or canals, visible therein when it is swelled with water; for upon compressing, we find the water dart in little threads to a good distance therefrom.

Of the voiding its excrements.

As to the voiding its excrements, we suppose it effected by the contraction of the circular muscles of the intestine, which are very numerous, and disposed in bundles. To see them, the intestine must be cut lengthways, the excrements
taken

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taken out, and the gut well opened; this done towards the base of the gland, to which the intestine is fastened, will be seen several large bundles of fibres, which surround the intestine, diminishing still in thickness as they recede further from their origin.

Of the respiration of muscles.

Muscles breathe the water much as fishes do, as appears from a little circular motion observable in the water near the heel of their shell; but they do not cast forth the water again each time they take it in, as fishes do, but blote themselves with it for a minute or two, and then throw it out all at once at the other end of the shell. This done they begin to take in fresh water, which they cast out as before, and this continue without ceasing. By this it appears, that the respiration of muscles differs somewhat from that of fishes; for these cast forth the water each time they take it in. It was in the crested muscles I observed this respiration, to which end they were laid flat, half of them under water, upon a fine sand; if they had been all hid in the water, one could neither have observed the little circulation of the water near their tail, nor even the expulsion of the water at the other end, by reason the proper motions could not in that case be produced on the surface of the water.

'Tis probable, that these fishes, when glutted with water, suddenly contract their muscles, to draw their shells one to the other, in order to compress their body, and thus force the water out; add that the muscle does not seem to breathe constantly, for having put some of them in large basons to have the conveniency of observing them better than in the river, I found them open from
time

time to time, but could not perceive that they inspired the water.

Of the diseases of muscles.

I have observed that river muscles are subject to diverse diseases, as the moss, itch, gangreen, and even *sphacelus*.

When the animal grows old, there gradually gathers on its shell a kind of grain, which is no other than a very short moss, like that upon stones. This moss may be the primary cause of all the diseases that befall muscles, by reason the roots thereof entering, as may seem, the substance of the shells, the little apertures occasioned hereby, afford passage to the water, which gradually dissolves them.

We sometimes find on their shells a slender tomentose, or downy kind of plants, finer than silk. This down, called by botanists, *alga*, may occasion the like disorders as the moss, and may likewise incommode the muscle much by obstructing this motion; for when one end of these plants is fastened to the shell, and the other to a stone or rock, the muscle will no longer be able to walk.

Tubercles are also often formed on the inner surface of the shell, which may be resembled to the itch, they probably arise from the dissolution of the shell, which coming to swell, heaves and thrusts forth the inner stratum, like the flesh which grows under the external lamina of a damaged bone, and makes it exfoliate. Some of these tubercles are as big as pease, and might be taken for pearls.

Lastly, the shells sometimes dissolve, by little and little, till becoming soft like membranes, they may be pulled in pieces. This seems to indicate
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Lastly, the shells sometimes dissolve, by little and little, till becoming soft like membranes, they may be pulled in pieces. This seems to indicate
that

that the shells are only membranes condensed and hardened; and the same likewise holds of bones, which in some diseases, become as soft as cloth.

An explanation of the figures, translated by J. M.

Plate VI. Fig. 1. A river muscle. of which the elastic ligament*, which makes the muscle open, is fastened externally to the heel of the shell.

Fig. 2. A sea muscle, of which the elastic ligament*, which makes the shell open, appears only internally toward its heel.

Fig. 3. An oyster, of which the elastic ligament*, which makes it open, is hidden between the two shells of the oyster.

Fig. 4. A muscle, which I have called crested, because it has a process* in the heel, in form of a crest.

Fig. 5. The tracks, which the muscles make and leave behind them, when they make their progressive motion in the mud or sand of the rivers.

VI. An extract of the observations made in December, 1705, by S. Bianchini, upon some fires seen on one of the Appenine mountains, by M. Cassini, jun. translated by Mr. Chambers.*

In the way from *Boulogne* to *Florence* through the district of *Pietra Mala*, flames are commonly perceived on the descent of a mountain. *S. Bianchini*, who had frequently observed them at a distance, resolving at length to go nearer and examine them, gives us the following account.

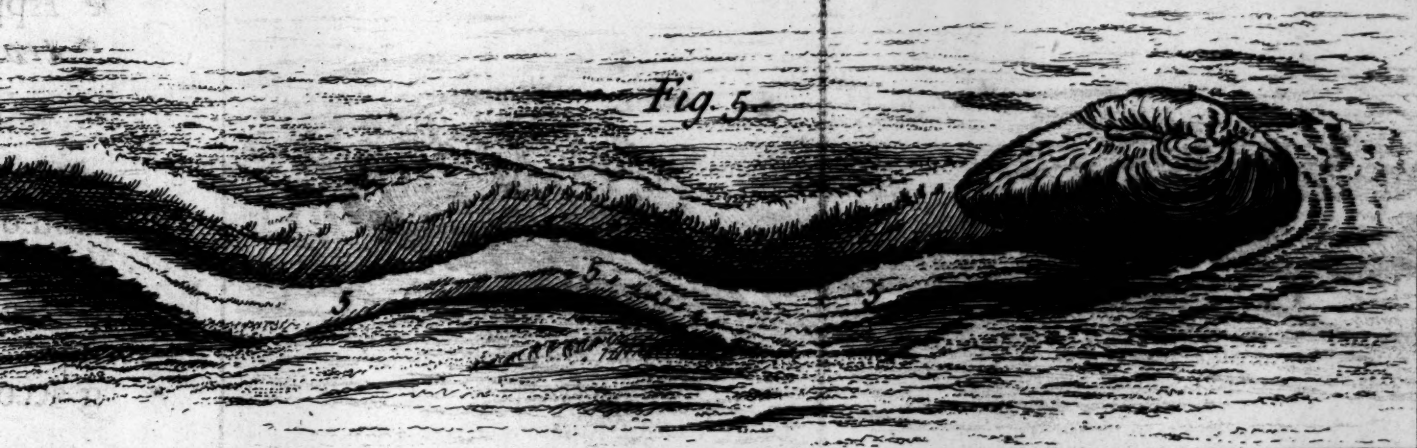
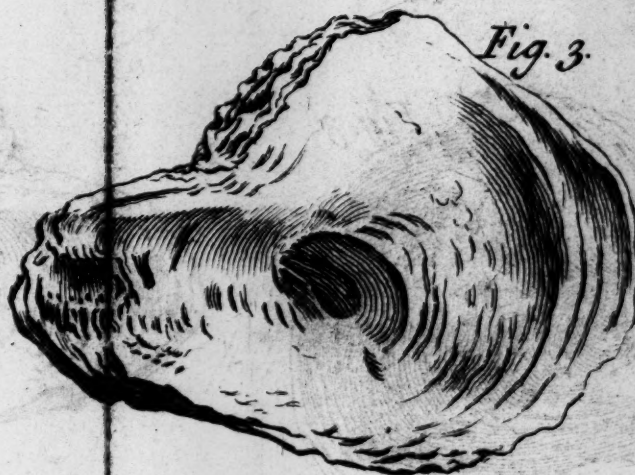
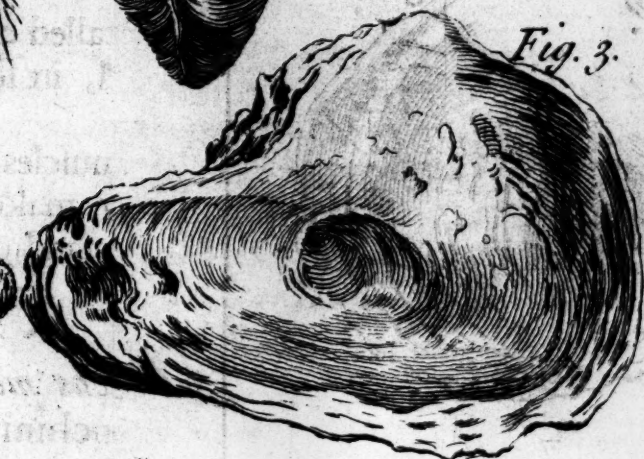
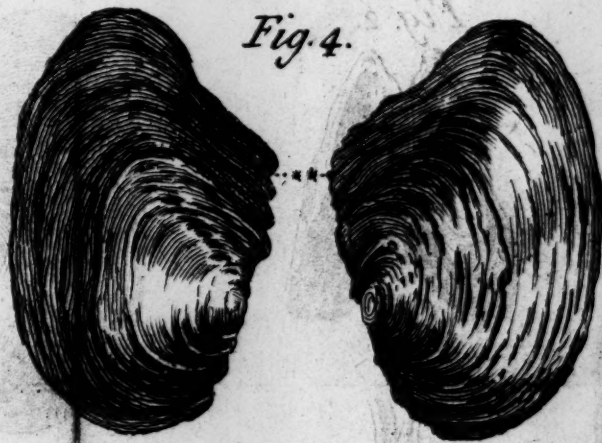
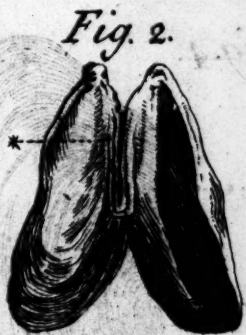
* Aug. 7, 1706.

I

After

Fig. 1.





After seeing a living flame arise without interruption, and without any matter to sustain it other than what it receives from the situation of the subterraneous places, found in the mountain of *Pietra Mala*, I make no doubt, but that the use of fire in our arts was 1st communicated and rendered durable by some of these natural mines, or springs of flame, observed in this mountain.— About the flames, I found snow and ice, not above 4 feet distant, which continued till about noon. I had several strangers with me, who had the same curiosity for examining, and we had a guide, who was to look to our horses at the top of the mountain of *Pietra Mala*, from whence we ascended on foot in a southern direction, for the space of about two miles, leaving the great road on the right, and descending the other side of the mountain by a narrow path, which ended in a plain, capable of being cultivated. In the middle of certain plowed grounds, we perceived a road, from whence arose several little flames, which seemed about half a foot above the ground, as if they arose from wood or coal. The place where these flames are found, is 8 *Roman* feet broad, and 16 long, and is capable of being measured as well as other parts of the field, it being easy to walk about, and even over the flame, without fear of falling into any hole or cavern, as on mount *Vesuvius*, the soil is here quite discontinuous, and differs in nothing from the rest, excepting that the vein of fire renders it somewhat firmer, and gives it more of an adust colour. I say the vein of fire, as not knowing how otherwise to call this unknown matter, which produces so many different flames dispersed here and there, in twenty several places, all within the compass of 130 feet square. I judged it need-

less to count each of them apart, since one may make flames arise from every spot of the whole compass, either by striking the ground slightly, with a stick or other body, or by throwing straw, paper, or other combustible matter thereon.

Yet when such combustibles were laid in a place at a distance from the flames, this did not hinder their taking fire, much as paper or linnen would have done, by casting them on a red-hot iron. In fine, we observe that one of the flames thus produced, after it had consumed the matter thrown on it, continued still burning, being fed with something else supplied by the soil.

We cast branches of thorns and other shrubs, gathered for the purpose, as we came on these flames, and found them burn as readily, as when thrown on a common fire, and observing some lumps of snow, not yet melted, within two feet of the flame, and finding pieces of ice under the snow at 4 feet distance from the flame, it not only put me in mind of what the poet said, speaking of mount *Ætna*, with its snows and fires; *Scit nivibus servare fidem*; but I had a mind to make the experiment of casting snow and ice into the flame, I did it accordingly, and found them dissolve in a moment, as if they had been cast upon a common fire. The flame however was not extinguished thereby; but, on the contrary, appeared more vivid, and spread more forcibly around.

While we made these experiments, all about the place we perceived a very agreeable odor, which seemed to issue from this burning soil, and was much like what arises upon burning some odoriferous wood, as calambouc. This odor was most sensible, when we stood opposite to the
sun

fun, or at some little breeze in her face, which increased the flame. I gathered some pieces of stones near the flame, and a handful of dust of the ground, which being rubbed against each other, produced flame, and yielded the like odor as that above-mentioned. These stones were so hot at first, that one could hardly bear them in the hand, and they preserved their heat for $\frac{1}{4}$ of an hour, and their smell much longer.

Having made these experiments, which appeared sufficient to satisfy our curiosity as to the first communication of fire, and may afford scope enough for philosophising upon so extraordinary an effect, we resumed our journey towards *Fiorenzola*.

Reflections on the foregoing observations.

This fire, observed by S. *Bianchini*, bears a near relation to that observed in *Dauphini*, by M. *Dieulamant*, whereof an account is given in the history for the year 1669.

There is no visible fuel of either of the flames, though this latter smells strong of sulphur, besides that it burns most in winter and moist weather. The two fires have this in common, that they are on the descents of mountains, and seem both to spring from the ground, without any apparent cleft, whereby to communicate with any cavern underneath. They are both of them likewise increased by moisture and cold weather, as may appear of the former by the snow cast thereon, which in thawing augmented the fire.

The chief difference lies in the smell, which in the one is sulphurous, and as it were aromatic in the other.

VII. *A dissertation on the barometers and thermometers, by M. de la Hire, jun. * translated by Mr. Chambers.*

We are much obliged to the philosophers of the last age, for having found a method of determining the several changes which befall the air, considered as a heavy and an elastic body : a discovery of the utmost importance, as it lets us into the reason of a multitude of phenomena, which had infinitely perplexed all former naturalists, and given rise to I know not what aversion in nature to a vacuum.

The celebrated *Galileo*, mathematician to the grand duke of *Tuscany*, was the first that observed, that water could only rise in a sucking pump to the height of 32 feet ; and that if the barrel of the pump exceeded this height, such excess would remain empty. The consequence he drew from this observation was, that nature had only an aversion for a vacuum at this height, which was a conclusion of a piece with the former philosophy.

Toricelli his disciple and successor made another experiment in the year 1643, taking a glass tube 4 feet long, and open only at one end, he filled it with mercury, and thus inverted it into another vessel full of mercury, the open end being downwards; the effect was, that the mercury in the tube sunk some 20 inches, and left a space in the top, which must necessarily be a vacuum.

This experiment was communicated by a letter from *Italy* to father *Mersenne*, a minim of *Paris*, who spread it thro' all *France*, till at length M. *Petit*, intendant of the fortifications, having im-

* Nov. 13, 1706.

parted

parted it to M. *Pascal*, they repeated the experiment together at *Rouen* in 1646, and found it perfectly agreeable to what had been notified from *Italy*; on which occasion M. *Pascal* made several other experiments, whereof he formed a book, which was published in 1647. The same year he received intelligences that *Toricelli* had suspected, that the weight of the air was the cause of the suspension of the mercury in the experiment above-mentioned, which led him to make some further experiments, by which he was confirmed in the sentiment of *Toricelli*, upon which he ventured to advance, that all those effects which had been attributed to nature's abhorrence of a vacuum, were only owing to the weight of the air, of which he made abundant proof in a treatise on that head.— Such were the epocha, and order of the several experiments, whereby this noble property of the gravity of the air were discovered: we proceed now to a description of the machines, contrived to discover its elastic power, beginning with the oldest, and proceeding according to the order of time.

Sanctorius, a celebrated physician of *Capo d'Istria*, bethought himself of a machine for discovering the different degrees of heat in feverish persons, which he called a thermometer, not considering, as we have all the reason to imagine, that the same machine might serve to shew the changes which befall the air as its bulk is increased by different degrees of heat; and that this would be of more advantage to the publick, than all its application to the business of medicine.

This thermometer consisted of two glass balls, fastened to a glass tube, bent at the bottom close to the lower ball; the upper ball, which had no communication with the external air, and all the
upper

upper part of the tube were full of common air, and the rest with part of the lower ball, which was open a top, were filled with *aqua secunda*.——

From this construction, it easily appears, that when the air of the upper ball was dilated further by a greater degree of heat, it would compress the *aqua secunda* in the tube, and oblige it to descend; and on the contrary, would let it rise again, when the same air came to be condensed into a less compass.

This instrument, tho' subject to great irregularities, was allowed very curious by all the learned, and put in use accordingly, till such time as the barometer was discovered, upon which a great defect was perceived in it, *viz.* That it acted likewise as a barometer, which might frequently destroy all its effect as a thermometer, for that the air in the lower ball, communicating with the external air, would act likewise on the liquor, and oblige it to rise or fall, according as it should be more or less heavy.

This discovery of the barometer proved a great blow to that of the thermometer; but it lasted not long. Some learned men at *Florence*, having contrived to make another, without the defect of the former. I have not been able to discover any date of the time when this latter was invented, except in *Otto Guerick's* book, intitled, *Experimenta Magdeburgica*, printed in 1672, wherein he mentions its being discovered about 30 years ago; and in *M. Petit's Dissertations Academiques*, printed in 1671, wherein the invention is attributed to the academy *Del Cimento*, who first gave the description and figure thereof in their *Saggi di naturali esperienze*.

This which should be called the *Florentine* thermometer, and which is that now most in use, being

being found very commodious for all kinds of experiments, as well as easily portable, is very simple in its make, consisting only of a glass ball, to which is fastened a tube hermetically sealed a top, whose diameter and length are so proportioned to the diameter of the ball, which, with a part of the tube, is to be filled with spirit of wine, as that the spirit, when dilated by the utmost degree of heat of our climate, may not quite fill the tube, nor when condensed by the utmost degree of cold, sink wholly into the ball.

But tho' this thermometer have great advantage, it had one considerable defect, which was, that we could not by means hereof compare the temperature of the air of one country with that of another, unless either one and the same thermometer were carried into both places, or several thermometers were divided to the same degrees of heat; but M. *Amontons* found a remedy for this, and made the thermometer truly universal, without changing any thing in its structure, by fixing a degree of heat, *viz.* that of boiling water, which is the same every where, whereby all the other degrees are to be directed, so that nothing further seemed desirable on this head; yet we find this year a further attempt of M. *Nuguet*, who has published a treatise, intitled *A New Discovery of a thermometer*, long sought for by the royal academy, free from the defects of other thermometers, and containing all the advantages found separately in all others hitherto invented.

Not to mention, that he had no grounds for quoting the academy, as engaged in such a research, we observe, that his thermometer is much like M. *Huygens's* barometer. It consists of a glass ball hermetically sealed, and full of air,
condensed

condensed by the cold of iced water, and of 4 cylindrical tubes, cemented to each other, which all together make only one single bent tube, the bend being downwards. This tube is filled like the double barometer, tho' with some precautions to be mentioned hereafter, by which means the space from the top of this tube to the middle of the first tube, is emptied of gross air; the rest of the space, as far as the middle of the third tube, which is over the bend in the other branch, being full of mercury. Lastly, over the mercury there is spirit of wine, as far as the middle of the fourth tube, to whose top the ball is fastened, which is full of air as well as the rest of this tube. —

Now, from this structure it easily follows, that as the heat increases, the spirit of wine must descend, and rise again as the cold returns, by reason the air in the ball, and part of the tube being dilated by the large heat, will oblige the spirit of wine to descend, and when condensed again by the cold, will leave the spirit at liberty to re-ascend; but neither this structure, nor manner of filling, I doubt, will appear so simple as that of the *Florentine* thermometer. Let us now see, how he fixes the ratio of his tubes, upon which the whole structure of his thermometer depends.

The proportion which he has taken between the tube, wherein the spirit of wine moves, and the other tubes, wherein the mercury is contained, and between the weight of spirit of wine and that of mercury is such, that when the liquor is arrived at the top of the third tube, which denotes the extreme heat of summer, the air in the ball will support 4 inches of mercury more than it will support when the same liquor arrives at the entrance of the ball, which denotes the greatest degree

degree of cold. The reason he gives for this proportion is, that the inclosed air will, by the highest degree of the summer's heat, be able to sustain 4 inches of mercury more than it sustains in the greatest winter's cold.

This passage, which is taken from his own writing, will afford room for several observations.

— 1st, That he says nothing of the diameter of the ball, wherein the air is inclosed, which it ought by all means to be considered, since by some experiments, which we have made, it appears, that different bulks of inclosed air, when exposed to the same degree of heat, will sustain the mercury at different heights; whence will arise a necessity of making those balls perfectly equal in all his thermometers, and the tubes equal likewise, or in the same proportion, which is almost impossible in the execution.

2dly, That he does not mention in what part of the earth the difference between the utmost summer's heat, and the winter's cold, will sustain 4 inches of mercury; and if, as we suppose, he means it at *Paris*, where the terms of heat and cold have lately been settled, yet to have any of these thermometers of use in other countries, they must be made there on purpose; those made at *Paris* being useless, by reason the greatest degrees of heat and cold, whereon the whole structure is found, change according to the country, which will induce a necessity of finding and fixing them, which is no small difficulty.

3dly, He should have expressed whether this air, which is able to sustain 4 inches of mercury more in summer than in winter, be inclosed in a state of condensation, since, from his account, it does not appear, that this air is condensed; and

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Yet that in the ball of his thermometer is condensed by the cold of freezing water, which leads us into a great difficulty, by reason the air whereon his thermometer is built seems of one kind, and that in the ball of another, and yet he seems to infer the effect which the latter will produce from that produced by the former.

4thly, That ice or freezing water will always be necessary for the making of his thermometers.

5thly, That regard must be had in making his thermometers to the different heights of the atmosphere, which occasion alterations in the body of air.

6thly, That notice must likewise be taken of the degree of dryness or moisture of the air, which may produce some alterations in his thermometer.

We proceed now to examine the precautions wherewith this thermometer is to be filled.— Ere the extremity of the ball be sealed, care must be taken that the spirit of wine contained in the tube, which is joined to the ball, correspond by its upper part to the degree of the scale of the common thermometer, which exactly expresses the degree of cold of freezing water, wherein they are plunged; and since he proportions the quantities of water and ice in such a manner, as that the cold arising from the mixture of the two, may suffice to make the liquor in the common thermometer sink to the 34th degree of its scale, he introduces the spirit into this tube, till such time as its upper extremity correspond to a point, which expresses the 33d degree of the scale of his thermometer.

'Tis evident, that in this method of filling his thermometer, he will always need one of the *Florentine*

rentine thermometers, and that it can never be universal, since only those made by the same common thermometer will be capable of being compared together, for supposing them equal in every thing else, the 33d degree of all the common ones will never express the same degree of cold, as this degree is not determined by any fixed uniform cause.

Thus much for the general difficulty in the structure of M. *Nuguet's* thermometer: I shall now proceed to a comparison of its effects, with those of a very good *Florentine* thermometer, which has been used by my father and self upwards of 30 years. — On the 25th of *June*, at a quarter past 2 in the afternoon, the sky being very clear, I exposed the *Florentine* thermometer to the sun, in a place perfectly free from wind, and that of M. *Nuguet* aside of it. This latter being placed very perpendicularly, I found it sink to $93 - \frac{1}{2}$, and a few minutes after rise again to $89 - \frac{1}{2}$, where it fixed, tho' still exposed to the sun, by which it appears, that this ascent could neither be owing to any cooling of the air during the experiment, since, in that case, it must have continued cooling, and consequently the spirit of wine have continued rising, unless we suppose the air to have only been cooled for a few minutes, in which case the sun's rays would have heated it again, and consequently the spirit must have sunk again, nor to a diminution of the effect of the sun's ray, by reason of the difference of his height above the horizon; since, after sinking to its lowest in a short time, when it began to rise again, it should have continued rising to the end of the experiment. The other thermometer rose to 86; after which there was no

sensible rise, tho' they both stood in the sun $25'$ —. After which, I removed them into a room, which was open to the east, and into which the sun could not play, where when they had stood long enough to settle, I found that of M. *Nuguet* risen to $78^{\circ}-\frac{1}{2}$, and the other sunk but to $64^{\circ}-\frac{1}{2}$; the difference therefore between that of M. *Nuguet*, when exposed to the sun, and in the room, was 11° , which are equivalent to 3 inches, 3 lines $\frac{1}{2}$; and the difference between the *Florentine* one in those two states, was $21^{\circ}-\frac{1}{2}$, which are equivalent to 7 inches, 3 lines $\frac{1}{2}$; consequently the latter was more sensible by half than the former.

It remains to be explained, why, upon exposing the new thermometer to the sun, it first sunk, and then rose again. The reason whereof is this, that the heat acting on the air and the spirit of wine at the same time, and the air being more susceptible of dilatation, it made the spirit of wine sink very readily at first, which is the only advantage that I can perceive in this new thermometer above others; but the spirit of wine at length warming and dilating it, compressed the air, and by means hereof gained 4 ——— in height, which is a proof, that the new thermometer ought to be considered, as composed of two others: one an air thermometer, as that of *Sanctorius*; and the other a spirit thermometer, as that of the *Florentine* academy, but which act against each other.

It may not be here amiss to advertise those unacquainted with the laws of dioptricks, that they are not to attribute the great rise of the *Florentine* thermometers, when exposed to the sun, to the spherical figure of their phials, which can have no effect thereon, more than if the spirit were exposed

exposed naked in any other vessel; for if by the curvity of the phial, the rays which fall on it be collected as they pass through the spirit, and thus heat that part they touch more strongly than they would otherwise do, they will desert another part of this spirit, and have no effect at all thereon; so that one compensates for the other.

VIII. *Reflections on the observations sent to M. le Comte de Pontchartrain, by F. Laval Regius professor of hydrography; by M. Cassini* *.

The situation of the observatory at *Marseilles*, in view of the horizon of the sea, gives *F. Laval* the convenience of observing the horizontal variations, which are commonly ascribed to the refractions of the visual rays.

Those which he has hitherto made, being corrected by the rule which we have given on occasion of the observations made on the mountain of *Notre Dame de la Garde de Toulon*, shew, that the horizon of the sea is seven small leagues distant from the observatory; and that the observatory is elevated 175 feet above the surface of the sea.

It is a thing that would merit being examined by the levelling made from the observatory to the water of the sea.

We have observed, that the apparent horizon of the sea is often seen lower than the true horizon, which is not always distinguished. For there is about the true horizon an edging of the surface of the sea, which is not distinguished from the sky. We judge of it by the summit of some islands, which are discovered beyond the

* March 13, 1706.

horizon,

horizon, and seem sometimes to be elevated into the sky above the apparent horizon of the sea, above which they make a reflection, which forms a reversed image of the mountain, contiguous to that of the mountain seen directly. These two images form a total one, divided into two equal and similar parts, by the true horizontal line, which is distinguished only by this appearance.

The variation of the apparent depression of the horizon hitherto observed by *F. Laval*, amounts to no more than a minute and half, the smallest having been $13' \frac{1}{2}$, and the greatest $15'$; but what is most observable is, that the greatest depression was observed when the sea was highest. The high sea ought rather to contribute to make the horizon of the sea appear higher; it is true, that at so great a distance the elevation of the sea by the tempest would not be sensible; and thus the difference of the apparent height of the horizon ought to be ascribed rather to the different temperaments of the air, which cause refractions in them.

We have sometimes seen the sun begin to appear in the morning above the horizon, a little elevated above the surface of the sea, of an oval figure, with its length according to the horizontal line: it grew larger by degrees upwards and downwards, and was divided into two by the horizontal line, till the upper part was detached from the lower, each part growing round, and forming at last, as it were, two suns; one of which was detached from the other, and receded from it, upon the common line, perpendicular to the horizon, till the lower one, which we suppose to be caused by the reflection on the sea, ceased to appear. *F. Laval* once observed the sun to set in form of a bonnet, detached from the whole
apparent

apparent horizon: this appearance might have been caused almost in the same manner, as we have explained those we have observed formerly.

The greatest declination of the sun, which F. *Laval* has found in summer, by the observations of the meridian heights of the sun, corrected by our elements of the refractions and parallaxes, comes very near that which we had determined, by the observations made 50 years ago, to be $23^{\circ} 29'$. By the winter observations, F. *Laval* found it smaller by $\frac{1}{2}$ a minute, which may be ascribed to some little difference of refraction, in the meridian height of the sun, which might be a little greater or less regular in winter. These are very useful observations, which ought never to be neglected. This little difference may come also partly from the divisions of the instruments, which are not so fine, but that we may doubt of some seconds in a great many degrees. In an instrument of 4 feet *radius*, 5 seconds occupy no more than $\frac{1}{48}$ part of a line, which is not very sensible. In the greatest instruments, we perceive a little trembling in the sun and other stars, which may be ascribed to the different degrees of the temperature of the air through which their rays pass; which leaves some little doubts in the observations. The observations near the horizon are subject to variations, by the different temperatures of the air, as appears by the use of telescopes, which in great heats shew an appearance of boiling in distant objects.

The objects seen together by a horizontal ray, are not always seen alike. We see from the same window, and from the same point, a mill at a distance, partly hidden behind a neighbouring building, sometimes quite elevated above the building, and sometimes, as it were, immersed
below

below it, and entirely hidden. Thus the proof that we make of instruments, by the direction of their *axes* to a distant object, is not always exempt from some little error; a visual ray, which passes thro' the same *medium*, not being always strait, or always crooked in the same manner, its curvature varying according to the different temperatures of the interposed air.

As for the observations on the *eclipses* of the first satelite of *Jupiter*, which F. *Laval* has continued to make as often as the weather permitted him, he admires their conformity to the calculations, inserted in the book of the *Connoissance de temps*, which they have taken care to compose, by using the corrections which I gave 8 years ago, which consist in subtracting four minutes of time from the epoch, in subtracting also one second from the 25 revolutions of the first satelite, and in augmenting the first inequality by $\frac{1}{30}$ part. These corrections very often reduce their calculations to the same minute that the observations give, which is a great confirmation of the elements upon which the calculations are founded. These elements depend not only upon the proper motion of the satelite, but also on the motion of *Jupiter* and his inequalities, on the motion of the sun, on the situation of the nodes of the satelite, and of those of *Jupiter*, on the mutual inclination of their orbits; and lastly, on the *hypothesis* used to determine the equation of time.

They are always numerous enough to leave room to question, whether any imperceptible errors, to which each of them is exposed, as all the elements of astronomy are, might not make some error considerable enough to require new corrections in a little time. We are always attentive to them, to endeavour to bring the theory
I
nearer

nearer and nearer to perfection; but hitherto we may be contented with the state in which it is, there being no other planets so well regulated, that we can determine the *phenomena* long before, in a time so exact, as the eclipses of this first satelite of *Jupiter* in his shadow.

We are not yet so well satisfied with the *hypotheses* of the other satellites, their second inequality is very different from that of the first; so that we cannot ascribe them by any means to the same cause, to which it seems at first, that the second inequality of the first satelite might be ascribed, which is that the light of the sun, which goes to the satelite, and is reflected to the earth, takes up considerably more time in making this way, when *Jupiter* and his satellites are much farther from the earth towards the conjunctions with the sun, than when they are nearer, as in the oppositions.

Having compared the observations of the eclipses of the first satelite of *Jupiter* made at *Marseilles*, with those which we made at the same time at *Paris*, the difference of the meridians, which results therefrom is $12' 10''$; having chosen the mean between the differences which are found, which is within about a second, the same that was found in the preceding years.

F. *Laval* has made a great number of observations on *Venus*, in her passage over the meridian, whence he has calculated her right ascension and declination, which are found conformable to those which we have determined by the observations made at the same time at *Paris*. He has made also some observations on mercury at the meridian, after which we communicated to him those which we have made. We had fa-

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vourable weather to observe *Venus* in the meridian on the day of her conjunction with the sun.

F. *Laval* has also observed the spots of the sun, which appeared during the year 1705, and he has determined the situation of them in the disk of the sun according to the method which we have practised in concert.

AN

An Explanation of the Terms of Art used in this Volume, which were not explained at the end of the first Volume.

A

A *Nimated needle*, one that has been touched by a loadstone.

Aqua secunda, the *aqua fortis*, which has been used in the dissolution of metals, and has thereby lost part of its strength.

Arch-stone, or *voussoir*, a stone cut in the shape of a wedge, to form the arch of a vault.

B

Bivalve-shells, such as open with two leaves; like folding-doors.

C

Cerebellum, the hinder part of the brain. It is of a firmer substance than the *cerebrum*, or the fore part of the brain, and of a yellow colour.

Cerebrum the brain. It is used in contradistinction from the *cerebellum*, or hinder part of the brain.

D

Denier, a small piece of copper money used in *France*.

Dura mater, a strong thick membrane, which lines the inside of the skull, and covers the whole brain.

E

Equilibrium, an equal weight of two bodies, when compared with each other. When two bodies are thus balanced, they are said to be *in equilibrio*.

Extrados, the outside of a vault.

I

Impost, the stone whereon the first arch-stone is laid.

Intrados the inside of a vault.

K

Key of a vault, the stone in the middle, which is perpendicular to the horizon.

M

Medulla oblongata, the medullary part of the *cerebrum* and *cerebellum* united.

O

Oesophagus, the gullet.

P

Pineal gland, a small gland in the third ventricle of the brain, so called from its resemblance to a pine-apple. *Descartes* supposed it to be the seat of the soul.

Pinion,

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Pinion, a smaller wheel, which plays in the teeth of a longer. The notches of a pinion are called leaves

Pltta, pila, or *silk-grass*, a sort of *ananas*, or pine-apple, which comes from *America*. They strip the leaves of their skin, and the inside is a bunch of fine silky fibres, of which the savages make their fishing lines and bow-strings, and the *Spaniards* fine stockings, and other curious works.

V

Vouffoir, see *arch-stone*.



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A D D E N D A.

THE following papers being ranged by M. Fontenelle, under the head of *astronomy*, were omitted in the preceding volumes; but as they conduce also to the improvement of *geography*, we have thought it proper to insert them in this place.

In the MEMOIRS of 1701.

VII. *A comparison of the principal phases of the eclipse of the moon Feb. 22, 1701, observed in different towns of Europe, related to the academy June 25, by M. Cassini.*

At Collioure.

The beginning could not be observed because of the clouds, but by comparing together the phases of the same magnitude before and after the middle of the eclipse,

The middle of the eclipse was found at	h	'	"
	11	27	20
The total end was observed at	12	36	57
Therefore the half of the duration was	1	9	17
And the beginning of the eclipse at	10	18	23

At Paris,

The clouds hindered the observing this eclipse, but we have found by the *series* of our triangles, that this city is more westward than *Paris* by 3' of an hour within about 1", which we shall here neglect; having therefore subtracted 3' from the preceding phases, the beginning of the eclipse must have been,

At <i>Paris</i> at	h	'	"
	10	15	23
The middle at	11	24	40
The end at	12	33	57

We

A D D E N D A.

We shall make use of these phases to find the difference of the meridians of the other places where this eclipse has been observed.

At Madrid by F. Ulboa and Caffani, jesuits.

	<i>At Paris.</i>			<i>Diff. of the merid.</i>		
The beginning at	h	'	"	h	'	"
	9	45	17	10	15	23
The end at	12	7	38	12	33	57
The duration at	2	22	21	2	18	34
The half	1	11	10	1	9	17
The middle	10	56	27	11	24	40

These differences of the meridians are so different from one another, that we cannot depend upon them. That which is taken from the end, differs from that which was formerly observed from 23 to 24 minutes.

At Cordova, by D. Pedro Antonio de Blancas.

The beginning	h	'	"
	9	45	30
The end	12	11	25
The duration	2	26	
The half	1	13	
The middle	10	58	26

The difference taken from the end seems to be preferable to that which is taken from the other phases.

At Pau, by F. Pallu, a jesuit.

The beginning	10	4	11
The end	12	20	12
The duration	2	16	
The half	1	8	
The middle	11	12	12

ADDENDA.

3

At Avignon, by F. Bonfa, a jesuit.

				<i>Diff. of the merid.</i>		
	^h	[']	["]	[']	["]	
The beginning	10	26	44	—	11	11
The end	12	43	28	—	9	31
The duration	2	16	44			
The half	1	8	22			
The middle	11	35	6	—	10	26

The difference taken from the end agrees well enough with the observations formerly made.

At Marfeilles, by F. Laval, a jesuit.

The beginning	10	28	30	—	13	7
The end	12	45	40	—	11	43
The duration	2	17	10			
The half	1	8	35			
The middle	12	37	5	—	12	25
By the satellites of μ					13	40
By the eclipse of $\odot$ 1699					12	28

At Straßburg, by M. Eifenschmid.

The beginning	10	37	37	—	22	14
The end	12	56	45	—	22	48
The duration	2	19	8			
The half	1	9	34			
The middle	11	47	11	—	22	31
By the eclipse of $\odot$ 1699					22	7
Difference of the meridians					21	21

At Nuremberg by M. Wurzelbaur.

The beginn. a little doubtful	10	50	*	—	34	47
The end doubtful	13	7	36	—	33	39
The duration	2	17	36			
The half	1	8	48			
The middle	11	56	48	—	34	8
By the satellites of μ					34	33

I

At

At Berlin, by M. Kirchius.

	Diff. of the merid.				
	^h	[']	["]	[']	["]
The beginning	10	15	36	44	13
The end	13	18	40	44	43
The duration	2	19	4		
The half	1	9	32		
The middle	12	9	8	44	28
By the middle of Tycho				44	32

At Kiel by M. Rehyer.

	^h	[']	["]	[']	["]
The beginning	10	45			
The middle	12	0		35	20
The end	13	15			
The duration	2	30			
Magnitude $6\frac{1}{4}$					

VIII. *A comparison of several observations of the eclipse of the sun, Sept. 23, 1699, made in different towns of Europe*

At Paris, in the royal observatory by Mess. Cassini and Maraldi.

The beginning of the eclipse at	8	15	0
The greatest darkness of 9 dig. 29 min.	9	27	0
The end	10	45	5
The duration	2	30	5

At Chatenay, by M. de Malefieu.

The beginning of the eclipse at	8	15
End	10	47
Duration	2	32

The meridian of *Chatenay* is very near the meridian of the observatory to the westward.

At

ADDENDA.

5

*At Marseilles by M. Chazelles, F. Feuillée, and
F. Laval.*

The beginning of the eclipse observed
at *Marseilles* } 8 30 3

By the figure at *Paris* 8 17 20

Difference of the meridians 0 12 43

The end of the eclipse observed at
Marseilles } 11 1 13

By the figure at *Paris* 10 49 0

Difference of the meridians 12 13

Mean between both 12 28

At Avignon, by F. Bonfa, a jesuit.

The beginning of the eclipse at *Avignon* 8 25 0

By the figure at *Paris* 8 16 40

Difference of the meridians 8 20

The end of the eclipse at *Avignon* 10 57 17

By the figure at *Paris* 10 47 30

Difference of the meridians 9 47

Mean 9 3

At Arles, by M. Davizard.

The beginning of the eclipse at *Arles* 8 24 30

By the figure at *Paris* 8 16 55

Difference of the meridians 0 7 35

The end of the eclipse at *Arles* 10 53 30

By the figure at *Paris* 10 46 10

Difference of the meridians 7 20

Mean 7 27

At Rochelle, by M. des Hayes.

The beginning of the eclipse at *Rochelle* 7 58 17

By the figure at *Paris* 8 13 0

Difference of the meridians 14 43

The end of the eclipse at <i>Rochelle</i>	^h	[']	["]
	10	23	7
By the figure at <i>Paris</i>	10	37	15
Difference of the meridians		14	8
Mean		14	26

At Genoa, by the marquis de Salvago and the marquis Alexander Grimaldi.

The eclipse was already begun at <i>Genoa</i> at	}	8	45	7
--	---	---	----	---

The beginning of the eclipse by the figure at <i>Paris</i>	}	8	19	40
--	---	---	----	----

Therefore the Difference of the meridians must be less than	}	25	27
---	---	----	----

The end of the eclipse at <i>Genoa</i>		11	19	42
--	--	----	----	----

The end by the figure at <i>Paris</i>		10	55	35
---------------------------------------	--	----	----	----

Difference of the meridians		24	7
-----------------------------	--	----	---

At Lyons, by F. S. Bonet, a jesuit.

The eclipse was already begun at <i>Lyons</i>		8	26	23
---	--	---	----	----

By the figure at <i>Paris</i>		8	16	0
-------------------------------	--	---	----	---

Therefore the difference of the meridians must be less than	}	10	25
---	---	----	----

The end of the eclipse at <i>Lyons</i>		10	57	20
--	--	----	----	----

By the figure at <i>Paris</i>		10	48	20
-------------------------------	--	----	----	----

Difference of the meridians		9	0
-----------------------------	--	---	---

At Madrid, by the duke d'Uzeda, the Abbe Scotti, and F. Kreza, a jesuit.

The beginning was not observed at *Madrid*, because of the bad weather.

The eclipse at $1\frac{1}{2}$ digit at <i>Madrid</i>		7	59	45
--	--	---	----	----

By the figure at <i>Paris</i>		8	23	0
-------------------------------	--	---	----	---

Difference of the meridians		0	23	15
-----------------------------	--	---	----	----

The end at <i>Madrid</i>		10	7	0
--------------------------	--	----	---	---

By the figure at <i>Paris</i>		10	29	10
-------------------------------	--	----	----	----

Difference of the meridians		22	10
-----------------------------	--	----	----

At

ADDENDA.

7

At Modena, by F. Fontana, a Theatin.

The beginning at <i>Modena</i>	h	i	"
	8	55	0
By the figure at <i>Paris</i>	8	20	45
Difference of the meridians	0	34	15
The end of the eclipse at <i>Modena</i>	11	33	20
By the figure at <i>Paris</i>	10	59	10
Difference of the meridians		34	10

At Strasburg, by Mess. Eifenschmid and Schmid.

The beginning of the eclipse at <i>Stras-</i>	}	8	41	27
<i>burg</i>				
By the figure at <i>Paris</i>		8	19	20
Difference of the meridians			22	7
The end of the eclipse at <i>Strasburg</i>		11	15	21
By the figure at <i>Paris</i>		10	54	0
Difference of the meridians			21	21

At Bologna, by Mess. Manfredi and Stancari.

The beginning at <i>Bologna</i>	8	56	30
By the figure at <i>Paris</i>	8	21	20
Difference of the meridians		35	10
The end of the eclipse at <i>Bologna</i>	11	36	49
By the figure at <i>Paris</i>	11	1	1
Difference of the meridians		35	49

At 12 miles from Parma to the west by F. Beccatelli, a jesuit.

The beginning	8	55	38
By the figure at <i>Paris</i>	8	22	0
Difference of the meridians		33	38
The end of the eclipse	11	39	54
Perhaps	11	36	54
By the figure at <i>Paris</i>	11	2	20
Difference of the meridians		34	24

At Nuremberg, by M. Würfelbaur.

The beginning of the eclipse	^h 8	['] 57	["] 14
By the figure at <i>Paris</i>	8	22	55
Difference of the meridians		24	19
The end at <i>Nuremberg</i>	11	35	56
By the figure at <i>Paris</i>	10	59	30
Difference of the meridians		34	26
The mean		34	22

At Kiel, by M. Reiher.

The end of the eclipse	11	33	0
By the figure at <i>Paris</i>	10	37	15
Difference of the meridians		35	45

At Gripswald in Pomerania, by M. Pyle.

It was observed to be $2\frac{1}{2}$ dig. eclipsed at	9	37	0
By the figure at <i>Paris</i>	8	44	15
Difference of the meridians		52	45
The end was observed at	11	40	30
By the figure at <i>Paris</i>	10	47	50
Difference of the meridians		52	40

IX. *The differences of the longitudes of several towns of France, taken from the observations of the eclipse of the sun of July 12, 1684.*

At Paris, by M. Cassini.

The beginning of the eclipse	^h 2	['] 26	["] 55	Taken from the following phases observed immediately.
The end	4	43	23	

At Aix in Provence, by M. le Prieur Gaultier.

The beginning at	2	54	30
The end	5	9	6

The

ADDENDA.

89

The hour of the end at the me- ridian at <i>Paris</i> by the figure	} ^h 4 ['] 55 ["] 0
Difference of the meridians	14 6 to the east
Difference of longitude	3° 31 30

At Avignon, by F. Bonfa.

The beginning	2 43 27
The end	5 4 37
The hour of the end at the me- ridian of <i>Paris</i> by the figure	} 4 56 3
Difference of the meridians	8 34 to the east
Difference of longitude	2° 8 30

At Lyons, by F. Hoste.

The end of the eclipse	4 59 20
The time of the end at the me- ridian of <i>Paris</i> by the figure	} 4 51 20
Difference of the meridians	8 0 to the east
Difference of longitude	2° 0 0

At Honfleur, by M. de Glos, professor of hydro- graphy.

The beginning of the eclipse	2 15 0
The end	4 35 30
The time of the end at the me- ridian of <i>Paris</i> by the figure	} 4 42 0
Difference of the meridians	6 30 to the west
Difference of the longitude	1° 37 30

At Pau, by F. Richaud.

The end of the eclipse	3 45 0
The time of the end at the me- ridian of <i>Paris</i> by the figure	} 4 54 0
Difference of the meridians	9 0 to the west

to

ADDENDA.

Before *Roses*, by *M. Chazelles*.

The beginning	2	40	0
The end	5	1	30
The time of the end at the meridian of <i>Paris</i> by the figure	4	57	30
Difference of the meridians	4	0	to the east
Difference of longitudes	1	0	

This last observation which shews *Roses* to be more eastward than *Paris* by a degree shews, that the meridian of *Paris* does not go to *Perpignan*, as in the maps of *France*, but that it passes thro' the western mountains of *Roussillon*.

This determination, which then seemed extraordinary, has been confirmed by the meridian of the observatory, being prolonged to these mountains in 1701.

M. Maraldi had used it in his map of *France*, published by the *Sieur Jaillet* in 1700. which is the first where the meridian of *Paris* has been directed thro' *France* to its true meridional bound in the *Pyreneans*.



F I N I S.

